PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. XIII. 1885–86. No. 121.

THE 103RD SESSION.

GENERAL STATUTORY MEETING.

Monday, 23rd November 1885.

The following Council were elected:—

President.

THOMAS STEVENSON, M. INST. C.E.

Vice-Presidents.

ROBERT GRAY, Esq.
A. FORBES IRVINE, Esq. of Drum.
DAVID MILNE HOME, Esq. of Milne-Graden.
JOHN MURRAY, Esq.
Professor DOUGLAS MACLAGAN.
The Hon. Lord MACLAREN.

General Secretary—Professor TAIT.

Secretaries to Ordinary Meetings.

Professor TURNER, F.R.S.
Professor CRUM BROWN, F.R.S.

Treasurer—ADAM GILLIES SMITH, Esq., C.A.

Curator of Library and Museum—ALEXANDER BUCHAN, Esq., M.A.

Ordinary Members of Council.

Rev. Professor FLINT, D.D.
Professor T. R. FRASER, M.D.
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Professor SHIELD NICHOLSON.
T. B. SPRAGUE, Esq., M.A.
Professor BUTCHER.
Professor M'KENDRICK.
THOMAS MUIR, Esq., LL.D.
Professor M'INTOSH.

By a Resolution of the Society (19th January 1880), the following Hon. Vice-Presidents, having filled the office of President, are also Members of the Council:—

HIS GRACE THE DUKE OF ARGYLL, K.T., D.C.L.
SIR WILLIAM THOMSON, LL.D., D.C.L., F.R.S., Foreign Associate of Institute of France.
THE RIGHT HON. LORD MONCREIFF of Tulliebole, LL.D.

BRITISH MUSEUM

Monday, 7th December 1885.

JOHN MURRAY, Esq., Ph.D., Vice-President,
in the Chair.

Two instantaneous Photographs—one of the explosion of Hellgate, and the other of the "Genista" yacht—were exhibited by the Astronomer-Royal for Scotland. The thanks of the Society were accorded to the Astronomer-Royal.

The following Communications were read:—

1. On the Motion of a Liquid within an Ellipsoidal Hollow.

By Sir William Thomson.

I have only recently noticed the propositions regarding fluid motion within an ellipsoidal hollow which form the subject of the present communication, and which, though obvious enough and remarkably interesting, do not seem to have been previously discovered.

PRELIMINARY.

I shall use the expression *homogeneous* rotation, or homogeneous molecular rotation, to designate the condition of a fluid in respect to rotation, when throughout it the amounts of its molecular rotation are the same and the axial lines parallel. This designation clearly includes the case of a rotating solid: but it is applicable of course to the more complex case of a fluid, in which irrotational motion is superimposed upon homogeneous rotation as of a solid. To illustrate the complex motion thus signified, consider the following three examples, of which (1) and (2) are included in (3):—

(1) Let a liquid kept in the shape of a figure of revolution, by a rigid containing vessel, be given in a state of homogeneous rotation round the axis of the figure. Let an impulsive rotation round a line perpendicular to this axis be given to the containing vessel. The instantaneous motion of the liquid, at the instant when the impulse is completed, consists of an irrotational motion superimposed on the given homogeneous rotational motion. The molecular rotation of the liquid does not generally remain homo-

geneous after the first instant. But I find it does continue homogeneous, however the containing vessel be moved, provided the shape be ellipsoidal; that is to say (for the present limited case), an ellipsoid of revolution whether prolate or oblate. The possible incident of the containing vessel being brought again to rest in any position after any motion round any succession of diameters perpendicular * to the figure of revolution is of course included.

(2) Given a rigid solid, with a hollow space of any shape not a figure of revolution, within it, full of liquid: solid and liquid all rotating homogeneously. Let the given rotation of the solid be impulsively brought to rest or to any other rotation, whether rotation with changed angular velocity round the same axis, or rotation round another axis. The instantaneous motion of the liquid, at the instant of the completion of the impulse, will be the resultant of the given homogeneous rotation, with an irrotational motion superimposed upon it; this irrotational motion of the liquid being the same as the motion which would be generated from rest by giving to the solid (whether impulsively or gradually) an angular velocity the same as that which, compounded with the first given angular velocity, produced the second angular velocity to which we supposed the first angular velocity of the solid to be suddenly changed.

In this second example, as in the first, the molecular rotation does not generally continue to be homogeneous in the altered condition in which the solid and liquid do not rotate as if all solid; but it does continue to be homogeneous, if the shape of the hollow is ellipsoidal.

(3) Given a spherical shell full of homogeneously rotating liquid, or a hollow of any shape in a rotating solid full of liquid, rotating homogeneously with the solid. By impulsive pressure at the boundary of the liquid, supposed now to be perfectly yielding, generate any prescribed normal components of velocity in all parts of the boundary. The effect will be to generate throughout the liquid an irrotational motion, the same as would have been generated had the fluid been given at rest. The resultant motion throughout will be the resultant of this irrotational motion, com-

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* Rotation of the containing vessel round the axis of figure has no effect on the liquid, and need not be included to complicate our considerations.

pounded with the given rotational motion. The irrotational motion in the case of the spherical hollow is of course easily calculated by the well-known spherical harmonic analysis for fluid motion. We consider here only the instantaneous motion, which exists at the instant when the impulse is completed. The infinitely more difficult problem of working out the consequences according to any prescribed conditions as to force, or as to changing shape, for the boundary, we do not follow at present. It will be fully followed up for the case in which the boundary of the liquid is spherical or ellipsoidal to begin with, and is constrained to be always exactly ellipsoidal. It will be proved that in this case the molecular rotation of the fluid remains always homogeneous. We shall see in fact that the geometrical "strain" is essentially homogeneous throughout a liquid contained within a changing ellipsoidal boundary, provided that the motion of the fluid be either wholly irrotational, or be at any one instant homogeneously irrotational. The homogeneousness of the geometrical strain being established, it follows from Helmholtz's fundamental principles of vortex motion, that the molecular rotation must continue homogeneous; its magnitude, when there is any stretching or contraction in the axial direction, varying inversely as the length of a line of the substance in this direction, and the axial direction varying so as to keep always along the same substantial line.

If there is the slightest deviation from exactness in the ellipsoidal figure, the homogeneousness of the rotation of the liquid is not maintained, and there is no limit to the amount of deviation from homogeneousness which may supervene in consequence of motions which may be given to the boundary, whether in the way of change of shape, or of motion without change of shape. Confining our attention for the present to motion of the boundary without change of shape, we find it interesting to remark that we may go on indefinitely increasing or indefinitely diminishing the energy of the fluid motion by properly arranged action in the way of moving the containing vessel. To continually increase the energy I believe the following rule may be correct, although I do not yet see a perfect proof of it. Suppose the containing vessel to be given at rest, and the liquid within it to have perfectly homogeneous rotation within the not exactly ellipsoidal hollow, watch it for a little time—it may begin to move or it may not. If it does not begin to move of itself, give it a very slight motion of rotation round any axis. Generally it will begin to move of itself, but it will not do so if the interior fluid motion fulfils a definite condition of kinetic equilibrium, and therefore if you do not see the containing case beginning to move of itself you must set it in motion. When you see it in motion, act upon it with a couple in any direction to do some positive work upon it, and then suddenly stop it. Left to itself now, it will certainly begin to move of itself. When you see it moving again, again do some work upon it gradually, and stop its motion suddenly. Go on incessantly acting according to this rule. The positive work done gradually will exceed the work undone suddenly each time, or at all events on the aggregate of a large number of times of repetition of the operation. Thus on the whole you will increase the energy of the fluid motion without continually giving kinetic energy to the containing vessel, as might be the case if you continued always to apply a couple in such a direction as to do positive work. Thus by going on long enough operating in the manner described we can present the containing vessel at rest with the liquid moving inside it with any amount of kinetic energy we please.

A simpler rule suffices for diminishing the internal energy. Simply place the containing vessel on flexible imperfectly elastic supports, and leave it to itself, or leave it to itself immersed in a viscous fluid. Watch it for a while till you see it moving; or if you do not see it beginning to move of itself give it a slight motion, then leave it entirely to itself. It will never come to rest unless for an instant, and the internal energy will diminish asymptotically towards zero.

I now proceed to prove the propositions regarding fluid motion in an ellipsoidal hollow referred to above.

I. *Irrotational motion of liquid in a rigid ellipsoidal shell.*

Given the motion of the boundary: required the motion of the contained liquid.

Let ϖ, ρ, σ, be the component velocities of the shell, and let ϕ be the velocity potential of the corresponding determinate * motion of the internal fluid. The component linear velocities of a point (x, y, z) of the shell are

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* Wm. Thomson, "On the Visviva of a Liquid in Motion," *Camb. and Dub. Math. Journal*, 1849; or Thomson and Tait's *Natural Philosophy*, secs. 312 and 317, example (3).

$$\rho z - \sigma y, \quad \sigma x - \varpi z, \quad \varpi y - \rho x \quad . \quad . \quad . \quad (1),$$

and the component linear velocities of (x, y, z) are

$$\frac{d\phi}{dx}, \quad \frac{d\phi}{dy}, \quad \frac{d\phi}{dz}, \quad . \quad . \quad . \quad (2).$$

If (x, y, z) be any point of the inner surface of the shell, the normal component of velocity (1) must be equal to the normal component of velocity (2); or in symbols

$$\left.\begin{aligned} &(\rho z - \sigma y)\frac{px}{a^2} + (\sigma x - \varpi z)\frac{py}{b^2} + (\varpi y - \rho x)\frac{pz}{c^2} = \frac{d\phi}{dx}\frac{px}{a^2} + \frac{d\phi}{dy}\frac{py}{b^2} + \frac{d\phi}{dz}\frac{pz}{c^2} \\ &\text{where} \qquad \frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1 \end{aligned}\right\} (3);$$

the axes of coordinates being taken as coincident with the axes of the ellipsoid at the instant considered; (a, b, c) being the three semi-axes of the ellipsoid; and p being the perpendicular from the centre to the plane touching the ellipsoid at $(x, y, z,)$. To satisfy this, assume

$$\phi = \mathrm{A}yz + \mathrm{B}zx + \mathrm{C}xy \quad . \quad . \quad . \quad . \quad (4),$$

and determine A, B, C, to fulfil the first of equations (3). We find that (3) is now satisfied by

$$\phi = \varpi\frac{b^2 - c^2}{b^2 + c^2}yz + \rho\frac{c^2 - a^2}{c^2 + a^2}zx + \sigma\frac{a^2 - b^2}{a^2 + b^2}xy\,^* \quad . \quad (5).$$

It is important to remark that this expression for ϕ satisfies the first of equations (3) independently of the second, from which we infer that with the same angular velocity of rotation, the motion of any portion of the contained liquid is independent of the magnitude of the ellipsoidal body, and is determinate from the ratios alone of the three semi-axes. From (4) we find for the velocity components :—

$$\left.\begin{aligned} u &= \rho\frac{c^2 - a^2}{c^2 + a^2}z + \sigma\frac{a^2 - b^2}{a^2 - b^2}y \\ v &= \sigma\frac{a^2 - b^2}{a^2 + b^2}x + \varpi\frac{b^2 - c^2}{b^2 + c^2}z \\ w &= \varpi\frac{b^2 - c^2}{b^2 + c^2}y + \rho\frac{c^2 - a^2}{c^2 + a^2}x \end{aligned}\right\} (6),$$

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* This solution is given in Lamb's *Fluid Motion*, sec. 102.

which is the explicit solution of the problem, so far as concerns merely the absolute velocity at any point of the fluid, which is generally considered far enough in the solution of a hydrodynamical problem. But it would be interesting in every case, and it is easy in this case, to complete it up to the determination of the position of every particle of the liquid at any time, and we may therefore go on to do so. Relatively to the axes of the ellipsoid let $(\mathfrak{x}, \mathfrak{y}, \mathfrak{z})$ be the coordinates at time t, of any particular particle $\mathfrak{P}$, of the liquid. The component velocities $(d\mathfrak{x}/dt, d\mathfrak{y}/dt, d\mathfrak{z}/dt)$ of the particle $\mathfrak{P}$, relatively to the ellipsoid are equal to the differences between the components (u, v, w), of the absolute velocity of $\mathfrak{P}$, and the corresponding components of the absolute velocity of an ideal point (x, y, z) rigidly connected with the ellipsoid, and coincident with $(\mathfrak{x}, \mathfrak{y}, \mathfrak{z})$ at the time t. These last components are

$$\rho\mathfrak{z} - \sigma\mathfrak{y}, \quad \sigma\mathfrak{x} - \varpi\mathfrak{z}; \quad \varpi\mathfrak{y} - \rho\mathfrak{x}, \quad . \quad . \quad (7).$$

Hence, and from (6), at the instant $(\mathfrak{x}, \mathfrak{y}, \mathfrak{z})$ coincident with (x, y, z) we have

$$\left.\begin{aligned}
&\frac{d\mathfrak{x}}{dt} = -a^2(\gamma\mathfrak{y} - \beta\mathfrak{z}) \\
&\frac{d\mathfrak{y}}{dt} = -b^2(\alpha\mathfrak{z} - \gamma\mathfrak{x}) \\
&\frac{d\mathfrak{z}}{dt} = -c^2(\beta\mathfrak{x} - \alpha\mathfrak{y}) \\
\text{where} \qquad &\alpha = \frac{2\varpi}{b^2+c^2}; \quad \beta = \frac{2\rho}{c^2+a^2}, \quad \gamma = \frac{2\sigma}{a^2+b^2}
\end{aligned}\right\} (8).$$

These are linear differential equations of the first order for determining $(\mathfrak{x}, \mathfrak{y}, \mathfrak{z})$ in terms of t. Denoting d/dt by δ, we may write them as follows—

$$\left.\begin{aligned}
\frac{\delta}{a^2}\mathfrak{x} + \gamma\mathfrak{y} - \beta\mathfrak{z} &= 0 \\
-\gamma\mathfrak{x} + \frac{\delta}{b^2}\mathfrak{y} + \alpha\mathfrak{z} &= 0 \\
\beta\mathfrak{x} - \alpha\mathfrak{y} + \frac{\delta}{c^2}\mathfrak{z} &= 0
\end{aligned}\right\} (9).$$

Operating now on this in the usual manner we find

$$\begin{vmatrix} \dfrac{\delta}{a^2}, & \gamma, & -\beta \\ -\gamma, & \dfrac{\delta}{b^2}, & \alpha \\ \beta, & -\alpha, & \dfrac{\delta}{c^2} \end{vmatrix} = 0 \qquad (10);$$

whence by expanding the determinant and removing the superfluous factor δ, we have

$$\frac{\delta^2}{a^2b^2c^2}+\frac{\alpha^2}{a^2}+\frac{\beta^2}{b^2}+\frac{\gamma^2}{c^2}=0 \qquad (11),$$

which gives

$$\left.\begin{array}{l} \delta=\iota\, a\, b\, c\sqrt{\left(\dfrac{\alpha^2}{a^2}+\dfrac{\beta^2}{b^2}+\dfrac{\gamma^2}{c^2}\right)} \\ \text{where } \iota \text{ denotes } \sqrt{-1}. \end{array}\right\} (12);$$

And from the second and third of (9) we have

$$\frac{\mathfrak{x}}{\dfrac{\delta^2}{b^2c^2}+\alpha^2}=\frac{\mathfrak{y}}{\alpha\beta+\gamma\dfrac{\delta}{c^2}}=\frac{\mathfrak{z}}{\gamma\alpha-\beta\dfrac{\delta}{c^2}} \qquad (13),$$

which gives

$$\mathfrak{y}=\mathfrak{x}\frac{\alpha\beta+\gamma\dfrac{\delta}{c^2}}{\dfrac{\delta^2}{b^2c^2}+\alpha^2}, \text{ and } \mathfrak{z}=\mathfrak{x}\frac{\gamma\alpha-\beta\dfrac{\delta}{c^2}}{\dfrac{\delta^2}{b^2c^2}+\alpha^2} \qquad (14).$$

In virtue of (12) we may take as the solution for any one of the coordinates, $\mathfrak{x}$ for example, as follows—

$$\left.\begin{array}{l} \mathfrak{x}=\mathrm{A}\cos\omega t \\ \text{where } \omega=2abc\sqrt{\left[\left(\dfrac{\varpi}{b^2+c^2}\right)^2+\left(\dfrac{\rho}{c^2+a^2}\right)^2+\left(\dfrac{\sigma}{a^2+b^2}\right)^2\right]} \end{array}\right\} (15);$$

and from this (14) gives

$$\left.\begin{array}{l} \mathfrak{y}=\mathrm{A}\dfrac{\alpha\beta\cos\omega t-\dfrac{\gamma}{c^2}\omega\sin\omega t}{\alpha^2-\dfrac{\omega^2}{b^2c^2}} \\ \\ \mathfrak{z}=\mathrm{A}\dfrac{\gamma\sigma\cos\omega t+\dfrac{\beta}{c^2}\omega\sin\omega t}{\alpha^2-\dfrac{\omega^2}{b^2c^2}} \end{array}\right\} (16).$$

These equations give explicitly the position of any chosen particle at any time, and of course it would be easy to find from them what the path is; but it is easier to do this from the unintegrated equations (8). Multiplying the first of these by α/a^2, the second by β/b^2, and the third by γ/c^2 and adding, we find

$$\frac{\alpha}{a^2}\frac{d\mathfrak{x}}{dt}+\frac{\beta}{b^2}\frac{d\mathfrak{y}}{dt}+\frac{\gamma}{c^2}\frac{d\mathfrak{z}}{dt}=0 \quad . \quad . \quad . \quad (17);$$

which proves that the orbit lies in the plane

$$\frac{\alpha}{a^2}\mathfrak{x}+\frac{\beta}{b^2}\mathfrak{y}+\frac{\gamma}{c^2}\mathfrak{z}=\mathrm{H} \quad . \quad . \quad . \quad (18),$$

where H denotes a constant.

Again multiplying the first of equations (8) by $\mathfrak{x}/a^2$, the second by $\mathfrak{y}/b^2$, and the third by $\mathfrak{z}/c^2$ and adding, we find

$$\frac{\mathfrak{x}}{a^2}\frac{d\mathfrak{x}}{dt}+\frac{\mathfrak{y}}{b^2}\frac{d\mathfrak{y}}{dt}+\frac{\mathfrak{z}}{c^2}\frac{d\mathfrak{z}}{dt}=0 \quad . \quad . \quad . \quad (19),$$

and integrating this we have

$$\frac{\mathfrak{x}^2}{a^2}+\frac{\mathfrak{y}^2}{b^2}+\frac{\mathfrak{z}^2}{c^2}=\mathrm{K} \quad . \quad . \quad . \quad . \quad (20),$$

where K denotes a constant.

This proves that the orbit lies on the ellipsoid (20); and we conclude that the orbit is the ellipse in which this ellipsoid is cut by the plane (18).

Going back now to the explicit fully integrated solution (15) and (16), we see that a particle of the fluid describes, relatively to the moving solid in which the fluid is contained, the ellipse specified by (18) and (20), according to the law of a single particle describing an ellipse under the influence of a force towards a fixed centre varying in simple proportion to distance from the centre.

Now the period of revolution of the containing shell round its axis of rotation (ϖ, ρ, σ) is $2\pi/\epsilon$

where $$\epsilon=\sqrt{(\varpi^2+\rho^2+\sigma^2)},$$

which is easily seen to be less than $2\pi/\omega$ [the value of ω being given by (15) above]. Hence considering the shell and contained liquid at any instant, and again at the later instant when the shell is again in the same position after a single complete revolution round the axis of its rotation, we see that, relatively to the shell, the liquid will have performed less than a complete period of its retrograde revolution by the difference $(2\pi/\omega - 2\pi/\epsilon)$; or by the fraction $(1 - \omega/\epsilon)$ of the period of the fluid relatively to the shell. In the extreme case of $a = b = c$ (the ellipsoid a sphere), $\omega = \epsilon$ and the retrograde motion of the fluid relatively to the shell is one complete revolution, in the period of the forward revolution of the shell: that is to say, the fluid is perfectly left behind, and remains unmoved while the shell turns. In the other extreme case of any one or any two of the quantities a, b, c being infinitely small, ω is infinitely small: that is to say, the fluid makes an infinitely small fraction of its retrograde revolution during the time of one turn of the shell in the direction which we are calling forward. It must not from this be inferred that the fluid moves very nearly, as if solid, with the shell. On the contrary, it experiences large distortion even in the first complete turn of the shell, and largely increasing to a maximum in the course of the first quarter period of the liquid relatively to the shell.

(*To be continued.*)

2. The Sacral Index in Various Races of Mankind. By Professor Wm. Turner, M.B., F.R.S.

In a paper on "The Index of the Pelvic Brim as a basis of Classification," which I read in September at the meeting of the British Association in Aberdeen, and published in the *Journal of Anatomy and Physiology*, October 1885, I briefly referred to variations in the length and breadth of the sacrum in different races of men, and pointed out that in some races the length exceeded the breadth, and that in others an opposite relation prevailed. These differences may be expressed numerically by computing a sacral index by the following formula:

$$\frac{\text{sacral breadth} \times 100}{\text{sacral length}}.$$

When the sacral index is above 100 the breadth of the bone is greater than the length, when the index is below 100 the sacrum is longer than broad. The following descriptive terms may conveniently express these differences in the relative length and breadth of the sacrum. As the Greek word ἱερον is the equivalent of the Latin sacrum, the term *dolichohieric* would signify a sacrum in which the length exceeded the breadth, whilst *platyhieric* would signify a sacrum in which the breadth exceeded the length.

In considering the modifications in the sacral index, as in the index of the pelvic brim, it is important to bear in mind that sex modifies the relative proportions, and that in women the sacrum as a rule is broader in proportion to its length than in men.

In working out the results at which I have arrived, I have measured a number of aboriginal skeletons, a few of which were brought home by H.M.S. "Challenger," but the greater number of which are in the Anatomical Museum of the University of Edinburgh. The detailed measurements of these skeletons are given in the Tables in Part ii. of my Report on the Human Skeletons, now in type for the Challenger Reports. I have also examined the literature of the subject so far as I have had access to it, and have analysed the observations on the length and breadth of the sacrum recorded by previous observers.

Observations on the length and breadth of the sacrum in Europeans of both sexes by Verneau, Görtz, and Garson have shown that in them the breadth exceeded the length. The mean sacral index for European men was 112·4, and for women 117, so that the sacrum in them was platyhieric.

In aboriginal Australians, on the other hand, the measurements of Keferstein, Barnard Davis, Spengel and myself on men have shown that the length of the sacrum as a rule exceeded the breadth. The mean of thirteen adult males was 98·5, *i.e.*, they were dolichohieric. In women again the sacrum was relatively broader, and the mean of nine adult females measured by B. Davis, Verneau, Garson, and myself, was 102·5.

Of the aborigines of South Africa the mean sacral index of three Bushmen, measured by G. Fritsch and myself, was 94; and of four Bushwomen, measured by Verneau, Görtz, and G. Fritsch, was 94·7. The mean sacral index of three male Hottentots, measured by Wyman and Fritsch, was 83·9, and the index of one female was 85. In six male Kaffirs, the mean sacral index was 92·8. In the Bush, Hottentots, and Kaffirs the sacrum was dolichohieric.

The mean sacral index of twenty-nine Negros, measured by Verneau, Spengel, B. Davis, and myself, was 106, *i.e.*, they were platyhieric; whilst the mean of ten Negresses, measured by Verneau, Fritsch, and myself, was only 98·8, so that they were dolichohieric. If these specimens give a correct average for the Negro race, it would seem as if the sacrum is not so broad in the women in proportion to its length as in the men.

In eight male Andamanese measured by Flower, the mean sacral index was 94; but in a single adult male measured by myself it was 114, and in a young male 106. In nine females measured by Flower, the mean sacral index was 106; in three adult females measured by me it was 111, and in a young female it was 96·5. The high index of my male Andamanese is probably exceptional, and the standard of the race is dolichohieric.

Three male Tasmanians, measured by Barnard Davis, had a mean sacral index 87, so that they were dolichohieric. In a single female the index was 104.

From measurements of the sacrum in Pacific Islanders, both Polynesians and Melanesians, or perhaps a mixture of the two races, made by Verneau, Barnard Davis, and myself, the mean sacral index was above 100, so that its proportions were platyhieric.

The number of sacra belonging to the Guanche and Esquimaux races, which have as yet been measured, is too few to frame an average on; but from a few specimens measured by Verneau and myself, it is probable that the mean proportions of this bone are platyhieric. In the Hindoos and Sikhs also my measurements would point to a platyhieric bone.

The information regarding the proportions of the sacrum in the Chinese or other Mongolians is too scanty to enable one definitely to classify this bone. From measurements of the sacrum in male Malays by B. Davis and myself, it is probable that the bone is dolichohieric.

From the observations of von Franque, Barnard Davis, and Verneau on the sacrum in North American Indians, and from the measurements of Verneau, Davis, and Garson on South American aborigines, it seems probable that the sacrum in them is platyhieric.

Although additional observations are needed on the sacrum in a larger number of individuals of many of the races, yet from the material now before me a provisional arrangement of the races of men into two groups, according as the sacral index is below or above 100, may be made. In constructing the following table, the proportions of the male sacrum have been especially relied on:—

DOLICHOHIERIC *Sacral Index below* 100.	PLATYHIERIC *Sacral Index above* 100.
Australians. Bushmen. Hottentots. Kaffirs. Andamanese. Tasmanians. Malays. Aïnos? Chinese?	Europeans. Hindoos. Negros. Melanesians. Polynesians. Guanche? Esquimaux? North American Indians. South American Indians.

If this table be compared with the one which I gave in my article on the Index of the Pelvic Brim in the *Journal of Anatomy and Physiology*, Oct. 1885, it will be seen that a dolichopellic brim is in many races conjoined with a dolichohieric sacrum. This is the case, for instance, in the Australians, Bushmen, Kaffirs, Andamanese, Aïnos, and Malays. Again, a platypellic brim is conjoined with a platyhieric sacrum in Europeans, American Indians, and probably Guanches, Esquimaux, and Hindoos. The pelves, which I arranged in the mesatipellic or intermediate division, partly belong as regards the proportions of the sacrum, as in the Negros and Melanesians, to the platyhieric group, and partly, as in the Tasmanians, to that with dolichohieric proportions. As the width of the pelvic brim is materially influenced by the breadth of the sacrum, it was only to be expected that a platypellic pelvis would have a relatively wide sacrum, and that a dolichopellic pelvis would have a relatively narrow sacrum.

When in a human pelvis the conjugate diameter of the brim is in excess of the transverse diameter, *i.e.*, has dolichopellic proportions, and when the proportions of the sacrum are dolichohieric, then in these characters it accords with the proportions of the pelvis in the apes and other mammals which possess five vertebræ in the sacrum. It is animalised or degraded in its characters, as compared with a pelvis which is both platypellic and platyhieric.

3. On a Case of Interlacing Surfaces. By Professor Crum Brown.

The simplest form of the interlacing surfaces as spread upon a plane is illustrated in fig. 1. It will be seen that we have here

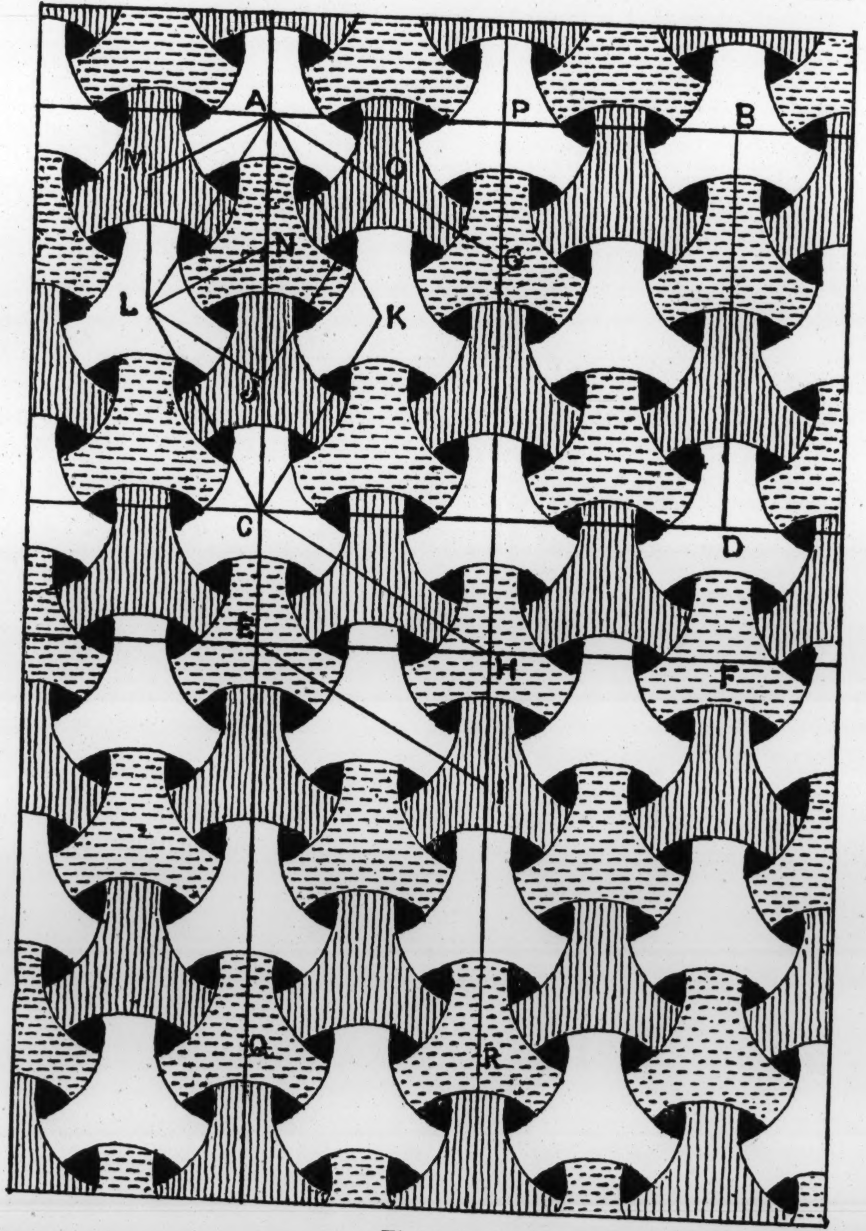

Fig. 1.

three sheets, differently shaded so as to distinguish them to the eye, but otherwise quite similar.

Each sheet is perforated by equal circular holes so arranged that any three neighbouring holes in the same sheet have their centres at the apices of an equilateral triangle. The radius of the holes must not be greater than half the distance between the centres of two neighbouring holes, otherwise the sheet would be cut into separate pieces, and must not be less than one-third of the said distance, otherwise there would not be room for the neck between two holes in one sheet to pass without crumpling through the chink caused by the overlapping of the holes in the other two sheets. In the figure the radius of the holes is about two-fifths of the distance between the centres.

The complex of three sheets is, as will be seen by inspecting the figure, a case of what Professor Tait calls *locking*. No two sheets are *linked* together; if any one sheet be abolished the other two come apart. Each sheet lies wholly above one of the other two, and wholly below the other.

The analogy of this complex to what we may call the Borromean * rings will be seen at once. In the Borromean rings figured below (fig. 2), each ring lies wholly above one of the other two, and wholly below the other, so that while all are inextricably locked together, no two are linked, and if any one is abolished the other two come apart.

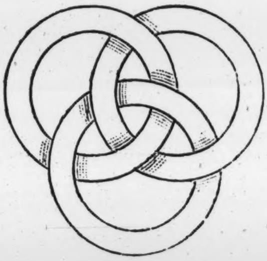

Fig. 2.

The complex of sheets may be applied to other surfaces besides the plane. Two other surfaces, viz., the cylinder and the anchor-ring, will be considered here.

To apply the complex to a cylinder or to clothe a cylinder with the interlaced sheets, we must cut the complex by two parallel lines, and roll up the strip thus cut out so that the two edges shall join and form what may be called the seam. But there must not be any peculiarity at the seam; the pattern must run through the seam without any discontinuity; therefore the two parallel lines must cut the complex in the same manner, so that each part of a hole divided by one line may find its exact continuation at the seam when the strip is rolled up.

* The interlocked rings shown above occur in the armorial bearings of the Italian family Borromeo.

There are, of course, an infinite number of ways in which such a strip may be rolled up into a cylinder. But in whatever way the cylinder is formed, if we cut it along a generating line, and unroll it, we may take the parallel edges of the flat strip as the two lines defining, on the plane complex, the particular cylinder. We may move these two parallel lines, parallel to themselves, retaining their distance from one another, in any way, and they will still represent the same cylinder, because we may form this flat strip by cutting the cylinder through any generating line. Two parallel lines will therefore represent a cylinder if the points in which they intersect a line at right angles to them are always similarly situated in reference to the complex.

The number of cylinders is obviously infinite, but they may all be grouped under two genera. For a part of a hole, cut off by one of the parallel lines, may, at the seam, find its continuation in a part of a hole, either first, in the same sheet, or second, in one of the other sheets.

In the first case we have three distinct sheets locked together. In the second we have only one sheet wound three times round the cylinder, and knotted. When we have three independent sheets we can colour or shade them independently, each having its own colour or shading, but when there is only one sheet this is not possible. In this case the only way of distinguishing the layers is by varying the colour, or shading, continuously as we go round the cylinder, so that after three turns we come back to the colour or shading with which we started. This has been done in the models exhibited.

We have assumed that the complex is flexible, we shall now assume that it is also extensible, so that we can draw it out in any particular direction, and make the circular holes into ellipses. We shall assume that any deformation may be produced without affecting the character of the complex as long as the topological relation of the layers is preserved. This extension is not of any use if we confine ourselves to cylinders, for there is no topological change produced by twisting a cylinder. The meaning of the extension will be seen when we come to apply the complex to an anchor-ring.

An anchor-ring can be made out of a cylinder in two ways. We may cut the cylinder by two planes at right angles to the axis, and bend the part thus cut out round so that its axis becomes the core of the anchor-ring; or we may cut the cylinder, and then widen out the two ends, and bend them over so that they may unite and form a seam not about the core, as in the last mentioned case, but about the axis of the anchor-ring. An anchor-ring has thus two seams—one a circle with its centre in the axis, and one a circle with its centre in the core—and it can be reduced to a cylinder, either by cutting the first, and, if we may coin the word, "unflyping," or by cutting the second, and unbending.

We see then that just as a cylinder can be represented by two parallel lines, so an anchor-ring can be represented by a parallelogram. The condition here is, that the parallel sides of the parallelogram cut the complex in precisely the same way. Such a parallelogram will in general represent two anchor-rings; we must therefore indicate which of the two pairs of parallel lines represents the seam about the axis, and which the seam about the core. To transfer from this plane plan to an actual anchor-ring—that is, to make a model such as those shown to the Society—we have only to remember that the four corners of the parallelogram represent the single point in which the two seams intersect; that the one pair of parallel sides represent the one seam, the other the other; and that lines parallel to these pairs of sides are to be measured on the anchor-ring, in the one case along the circumference of a circle about the axis, in the other case along the circumference of a circle about the core.

As there are two genera of cylinders, one knotted and one locked, so there are four genera of anchor-rings. 1st, locked about the axis and locked about the core; 2nd, locked about the axis and knotted about the core; 3rd, knotted about the axis and locked about the core; 4th, knotted about the axis and knotted about the core. Of these only the first, which is not knotted at all, consists of three distinct sheets; the second is reduced to a locked cylinder by cutting it along a seam about the axis, to a knotted cylinder by cutting it along a seam about the core; in the third these relations are reversed; in whichever way the fourth is reduced to a cylinder, a knotted cylinder is produced.

It is worthy of notice that anchor-rings of the fourth kind have necessarily "helicoidal asymmetry," a ring of this kind is necessarily enantiomorph to its mirror-image.

In the plate the lines AB, CD represent a locked cylinder; AB, EF a knotted cylinder; AQ, PR the smallest locked cylinder; CD, EF the smallest knotted cylinder; the parallelogram ABCD an anchor-ring of the first kind; AGCH one of the second kind, if AC and GH represent the seam about the axis; one of the third kind if these lines represent the seam about the core; AGEI one of the fourth kind; AKLC the smallest ring of the first kind, with one hole in each sheet; AOLJ the smallest ring of the second (or of the third) kind, with two holes altogether; MALN the smallest ring of the fourth kind, the smallest ring indeed of any, having only one hole altogether.

We have hitherto considered the complex as composed of *perforated* sheets locked together, or of a *perforated* sheet knotted, but there is another way in which it may be imagined.

We saw that the smallest circular hole had a radius of one-third of the distance between the centres of two neighbouring holes in the same sheet; but we can make the hole smaller if, instead of making it circular, we make it hexagonal. There is then no waste space; every part of the complex is composed of two layers, one over the other. Now we may suppose this hexagonal boundary to be, not the edge of a hole, but a line of intersection, where the surface, instead of ceasing, disappears between the two other sheets.

The knitted model exhibited illustrates this form of complex.

4. On the Foundations of the Kinetic Theory of Gases.

By Professor Tait.

(*a*) On the ultimate average Distribution of Energy among Systems of smooth Colliding Spheres.

The following note was written at the instance of Sir W. Thomson. It is directed mainly to one extremely important point connected with the Kinetic Gas Theory; and is designed to show to the ordinary reader the nature of the investigation, and of the real evidence for the result, without the imposing array of symbols which is usually marshalled in papers on that Theory.

1. Prop. VI. of Clerk-Maxwell's well-known earliest investigation of this subject (*Phil. Mag.* 1860, I. 25) deals with two systems of colliding spheres (those in each system being equal to one another). Their coefficient of restitution is unity, and they rebound without loss of energy from the walls of the containing vessel. His statement is that, *after many collisions*, the average kinetic energy of a sphere is (ultimately) the same in each of the colliding systems.

Particular stress must be laid on the words I have put in italics, because they form the basis of the whole theory. Without collisions there could be no law of any kind; the arrangement would be, and would continue to be, an absolutely haphazard one about whose character we could not possibly reason. And only after *many* collisions, among great numbers of spheres, can there be any approach to a statistical finality of arrangement.

The theorem is undoubtedly true, provided the number of spheres in *each* system be extremely large, while those of one system are not extremely numerous in comparison with those of the other; but the proof given by Maxwell has more than one very objectionable feature.

2. The chief of these is the *assumption* (for it is nowhere justified) that the transference of energy from system to system can be calculated without taking account of the mode of its distribution among the particles of either system. This assumption enables Maxwell to reduce the question to the treatment of the consequences of a single impact between a P and a Q (these letters represent the *mass* of a sphere of either system); each having the average energy of the system to which it belongs, and being thus regarded as typical of its system. It is typical of all the impacts between a P and a Q (here called *simultaneous*) which take place in the average time which elapses between any two successive impacts of a particular P on some Q or other.

The elegance of the investigation is farther enhanced by the additional assumption that, in obtaining the results of this typical impact, the original directions of motion of the P and Q may be taken as at right angles to one another.

The basis for *this* assumption is, apparently, a previous proposition, which shows that the mean square relative velocity of a P and a Q is the sum of the mean square speeds of the Ps and the Qs.

This proposition is true, under the conditions assumed, and can be proved as below,* by a process much easier for the beginner than that of Maxwell.

But if this assumption were justifiable, a farther application of the same principle should bring out that P and Q (being treated as typical particles after, as well as before, colliding) also *separate* after impact with motions in directions at right angles to one another. For, in the cases here considered, an instantaneous reversal of each velocity would make the whole system retrace its motion.† Maxwell's formulæ, however, show that in general the directions of motion after the typical impact are *not* at right angles to one another. It is clear that this objection is fatal to the method.

But there is yet a farther objection. In interpreting the result obtained in virtue of the assumptions already described, Maxwell exaggerates the rate of equalisation of the average energy in each system by treating the question as if every P impinged on a Q in the "simultaneous" sense above spoken of; and thus ignoring the almost incomparably more numerous particles of each system which, during the very short period contemplated, were either free from impacts or impinged on fellow particles.

So that, finally, he arrives at the very startling conclusion that the difference of the average kinetic energies of a particle from each system is reduced at *every* group of simultaneous impacts in the ratio $(P-Q)^2/(P+Q)^2$. Thus the equalisation of average kinetic energy in the two systems would be instantaneous if the masses of the Ps and Qs were equal!

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* The mean value, of the square of the distance of any point on a sphere from an internal or external point A, is the sum of the squares of the radius of the sphere and of the distance of A from the centre. Divide the spherical surface into pairs of elements by double cones, of very small angle, whose vertices are at the centre. For each pair of these the theorem is obviously true. Hence if the speeds of two points be p and q, their mean square *relative speed* is p^2+q^2. From this the above statement follows at once; provided that all directions are equally likely for each amount of speed.

† Here we meet with a quasi-metaphysical difficulty, which must be mentioned in passing. For, it may be said, since there is perfect reversibility, the mere instantaneous reversal of a state which is approaching finality will give a state whose tendency is to *depart from* finality, *i.e.*, to get back to the exact reverse of its original condition. True, and most important, but *not* fatal to the conclusion; unless an infinite time has elapsed since the start. For, when the reversal has brought the system back to the same configuration as at starting, but with velocities reversed, it is a *new* departure:—which will lead towards, but never to, its own state of finality.

Sir W. Thomson has suggested that part of Clerk-Maxwell's MSS. must have been, by mistake, omitted in sending to press. But I do not think that this idea is confirmed by a careful examination of the text.

Thus it appears that the objections to Maxwell's proof depend, in the main, upon his having commenced too soon to simplify by means of averages. It does not appear that his method, when the objectionable assumptions are put aside, can be applied in any simple manner (see *Proc. R.S.E.*, Dec. 15, 1884). But the investigation may be conducted very simply, as follows, by a method which shows clearly, at every step, what assumptions are made and how they are to be justified.

3. When two impinging spheres are of equal mass, their velocities in the direction of the line joining their centres at impact are simply interchanged. Hence the impact of a P on a P alters (in general) the *distribution* of kinetic energy in the system, but does not alter its average value per particle. These results are, of course, obvious; but they show how it comes about that one particle among a very great number of equal ones (which originally had equal speeds, let us say) may attain any speed, however great; while others may be brought (for a brief period) to rest. This has always, in my experience, formed a serious difficulty to beginners. But, as will be seen, it is a necessary characteristic of *statistical* uniformity.

To take a simple case, this will occur whenever a special particle always impinges on others, so that its own direction of motion is perpendicular to, and that of the other along, the line of centres at each impact. For it thus gets at each impact the whole energy of the two, and it *might* go on doing so till it had reduced all the others to rest. No doubt the acquirement of large speeds is common enough, but only for a few at a time even among a very large group of particles. It will presently be shown that there is a special distribution of relative position and velocity among the particles; towards which there is, on the whole, an approximation, though not necessarily a continuous one. In this special distribution, all speeds occur, but the number of particles which have either high or low speeds (as compared with the *mean square* speed) is a very small fraction of the whole group. After a very long period the final state will be one of irregular fluctuation about this "special" distribution, a fluctuation confined within limits which are (relatively) narrower as the whole number of particles is greater. This special distribution, of course, is that of uniform number of particles, zero of average velocity in every direction, and "error-law" distribution of equal amounts of kinetic energy, in every region of given volume large enough to contain a very great number of particles. Of this proposition satisfactory proof has been given by Maxwell; but we may obtain it very simply by the following considerations.

4. The tendency is to *levelling* all round. The only things to be levelled are the distribution of the whole momentum in each direction, and the distribution of energy among the various velocities. The first depends on direction cosines, the second on their squares. From a point, lay off lines representing the velocities of the various particles. The ends of these lines of lengths r to $r+dr$ must be *uniformly* spread in the volume $4\pi r^2 dr$. This secures that the momentum is equally and similarly distributed in all directions. The energy condition requires that, if there be a final state at all, the number of ends in unit of that volume shall be subject to the "error-law," *i.e.*, expressed by Ae^{-r^2/a^2}. This law is the only one which (when the momentum condition is secured) does not make the calculated number of "ends" in a given volume dependent upon our choice of rectangular axes. We have now to show *how* the collisions tend to produce this result, and also to prove that they tend to maintain it.

Impacts on the containing vessel do not alter r, and thus can shift only the position of an "end" on the spherical surface of which r is the radius. And the impact of two equal particles (as we saw above), does not alter the *distribution* of velocity along the line of centres, nor in any direction perpendicular to it.

Hence impacts, in all of which the line of centres is parallel to one common line, produce no change in the arrangement of velocity-components along that line, nor along any line at right angles to it. But there will be, in general, changes along every other line. It is these which lead gradually to the final result, in which the distribution of velocity-components is the same for all directions.

When this is arrived at, collisions will not, in the long run, tend to alter it. For then the uniformity of distribution of the spheres in space, and the symmetry of distribution of velocity among them, enable us (by the principle of averages) to dispense with the only limitation above imposed, viz., the parallelism of the lines of centres in the collisions considered.

5. When a P impinges on a Q, let u and v be their velocity-components (measured towards the same parts) in the line of centres at impact. Let these be changed by the impact to u' and v' respectively. Then the ordinary text-book result is

$$P(u'-u) = -\frac{2PQ}{P+Q}(u-v) = -Q(v'-v).$$

From this we deduce immediately

$$P(u'^2-u^2) = -\frac{4PQ}{(P+Q)^2}\left(Pu^2 - Qv^2 - (P-Q)uv\right) = -Q(v'^2-v^2).$$

This shows the amount of energy transferred between the P and the Q at one impact.

6. To obtain an average from this we begin by assuming that the Ps and Qs are thoroughly mixed, and are *separately* in the "special" condition of § 4. Of course, this implies that there is a very large number of particles of *each* kind. We also assume, what will probably on consideration be granted, that the mutual actions of the Ps alone, and of the Qs alone, still tend to preserve in each system this "special" state; or to restore it if it should be disturbed by the action of some Ps upon Qs. This is based partly on the uniform mixing of the Ps and Qs, partly upon the small percentage of each system which is involved in any "simultaneous" collisions. If this be granted, it is clear that we may assume that for a great number of *simultaneous* (§ 2, above) impacts of Ps on Qs, the average value of uv is at least approximately zero.* The reader must bear in mind that u and v are velocity-components parallel to the line of centres, which may have *any* direction. But we also see that we may now look on the average value of $Pu^2 - Qv^2$ as being two-thirds of the

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* There is no inconsistency between the two expressions above, viz., "great number of simultaneous impacts," and "small percentage of each system which is involved in any simultaneous collisions." For we must remember that the whole number of particles is *very great;* and even a "small percentage" of a very great number may itself be "a great number."

excess of energy of the *impinging* Ps over that of the corresponding Qs. Similarly the average value of $P(u'^2 - u^2)$ may be looked on as two-thirds of the increase of energy of the *impinging* Ps, and so forth. But the assumptions above enable us to say that, because there are many "simultaneous" collisions, the average value of Pu^2 for the *impinging* Ps is the average value for *all* the Ps. &c. Then our equation shows that, with the above assumptions, the *impinging* Ps lose energy on the whole if, and only if, their average energy is greater than that of the Qs they impinge on. But such gains or losses of energy distribute themselves through the systems of Ps and Qs separately; so that, on the whole, there is transference of energy from the Ps to the Qs so long, and only so long, as the average energy of a P is greater than that of a Q. Thus there is an approach (persistent in the long run, but not in general continuous) to equality between the average energy of a P and a Q; and, with these assumptions, Maxwell's proposition is undoubtedly true.

7. We may, in passing, make an approximation (of a very rough kind) to the rate at which this equalisation goes on, as follows:—

Let ϖ be the whole number of Ps,
ρ ,, ,, Qs;
ν the number of impacts between a P and a Q, in
τ the average interval which elapses, for each one P, between impacts on Qs. Let $Pp^2/2$ be the average energy of a P, $Qq^2/2$ that of a Q. Then our equations give (omitting the numerical factor $\frac{1}{3}$)

$$\varpi d(Pp^2) = -\frac{4PQ\nu}{(P+Q)^2}(Pp^2 - Qq^2)\tau = -\rho d(Qq^2).$$

Writing x for Pp^2, y for Qq^2, and N for $PQ\nu/(P+Q)^2$, this becomes

$$\varpi\dot{x} = -N(x-y) = -\rho\dot{y},$$

whence

$$Pp^2 = x = A - \rho B\epsilon^{-et}$$
$$Qq^2 = y = A + \varpi B\epsilon^{-et},$$

where

$$e = N\frac{\varpi+\rho}{\varpi\rho}.$$

8. It is foreign to my present purpose to enter into the calculation of the values of ν and τ, which depend on the diameters of a P and a Q, and the average distance between the centres of any two

proximate particles, as well as on the average speeds of a P and a Q. (This calculation can be effected by a simple method closely analogous to that in § 4 above. See the paper immediately following this.) But it is very much to my purpose to look back on what precedes, so that we may clearly see what assumptions *had* to be made in order that our results might be such as they are.

And we see at once that the investigation, from the point of view taken, would have been barren of interpretable result had it not been for the assumptions by which we [so far]* justified (in § 6) the statements:—

(*a*) Average value of $uv = 0$.

(*b*) Average value of $Pu^2 - Qv^2 = \frac{1}{3}(Pp^2 - Qq^2)$.

This last may be considered as including—

(*c*) Average value of $P(u'^2 - u^2) = \frac{1}{3}P(p'^2 - p^2)$ divided by the ratio, of the number of Ps which impinged on Qs, to the whole number of Ps.

Now these assumptions were themselves justified solely by the understood "special" state of the Ps and Qs separately; and by the "equalising" property, in virtue of which each system, so far as its own internal actions are concerned, tends in the long run to that special state. We are not warranted in concluding that either (*a*) or (*b*) would hold true unless the separate systems tended by their own internal actions to the "special" state. Thus, suppose the Ps to impinge on one another, and on the Qs; but the system of Qs to have no internal impacts. This would be the case if the Qs were mere points; *i.e.* particles of diameters infinitely small in comparison with the average distance between two proximate ones. Nothing above, so far at least as we have developed it, warrants us in concluding that the Qs will tend to a special state, and, therefore, acquire the same average energy as the Ps. Obviously, if the Qs were in a great majority, they would not only not themselves assume a special state, but would also tend to prevent the Ps from ever doing so. Think of Le Sage's ultramundane corpuscles and their effects. Still less should we, under such limitations, be justified in the corresponding conclusions with regard to a mixture of three or more systems, each of equal spheres, which form the subject of the corollary to Maxwell's Prop. VI.

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* [Inserted Jan. 8, 1886.] I hope to show at the next meeting of the Society that, though neither of these assumptions is correct, Maxwell's Theorem is rigorously true. Neither in Maxwell's paper nor in this has any account been taken of the fact that collisions are more frequent as the *relative* speed is greater. This consideration affects only *numerically* the results of §§ 7, 9, 10 above, and does not interfere with the argument based on them.

We now pass to a different, but closely connected subject.

9. Boltzmann's generalisation of the corollary to Clerk-Maxwell's Theorem, in which it is asserted that, after numerous collisions, the average energy is the same for each degree of freedom of the similar and equal complex particles of a colliding system, has proved a stumbling-block in the way of the Kinetic Theory, by being apparently irreconcilable with one or other of two experimental facts, (1) the value of the ratio of the specific heats of a gas, (2) the complexity of the spectrum of a self-luminous gas.

It appears from the above that there is an immediate mode of escape from this difficulty, provided the complex particles be so constructed that there is not perfect access for collision between *every* degree of freedom of one particle and *every* degree of freedom of every other. We cannot further consider this here, but pass for a moment to another view of the subject.

For, even if Boltzmann's Theorem were true without this condition, we must not at once conclude that a gas cannot consist of such complex particles. Every experiment shows that some, at least, of the quicker vibrating parts of the particle must be constantly losing energy by uncompensated radiation; and when the whole is, in spite of this, kept at what we call constant temperature, the requisite supply of energy comes in a translational form by impacts on the walls of the vessel. We may form an approximation, to what would then happen, by the simple expedient of supposing the coefficient of restitution to be less than unity for some of the degrees of freedom. In such a case the equations of § 5 become, respectively,

$$\mathrm{P}(u'-u) = -\frac{(1+e)\mathrm{PQ}}{\mathrm{P}+\mathrm{Q}}(u-v) = -\mathrm{Q}(v'-v)$$

and

$$\mathrm{P}(u'^2-u^2) = v\frac{2(1+e)\mathrm{P}}{(\mathrm{P}+\mathrm{Q})\cdot}\left\{\mathrm{P}u^2-\mathrm{Q}v^2+\frac{1-e}{2}\mathrm{Q}(u^2+v^2)\right\}$$

$$\mathrm{Q}(v'^2-v^2) = \frac{2(1+e)\mathrm{PQ}}{(\mathrm{P}+\mathrm{Q})^2}\left\{\mathrm{P}u^2-\mathrm{Q}v^2-\frac{1-e}{2}\mathrm{P}(u^2+v^2)\right\}.$$

We have omitted terms in uv from each of the right-hand sides; taking for granted that, in this case also, we may treat their mean value as nil if the number of colliding pairs is sufficiently great, and if the equalising process goes on in each system. These equations, with the proper alterations, apply to the internal impacts in the systems of Ps and Qs separately. The values of e may be different for a P and P and a Q and Q; but from them the value for a P and Q can be calculated. [Hodgkinson, *B. A. Report*, 1834.]

10. In particular there is a specially interesting case when $e = 1$ for a P and P, and for a P and Q; but $e < 1$ for a Q and Q. Here it is easy to see that the equations of § 7 are modified to

$$\begin{aligned} \varpi\dot{x} &= -\mathrm{N}(x-y) \\ \rho\dot{y} &= \mathrm{N}(x-y) - \mathrm{N}'y \end{aligned}$$

where N′ depends upon e and upon the frequency of impacts among the Qs.

[This, and all the equations which correspond to questions of the kind above proposed, are of the type

$$\left.\begin{aligned} \dot{x} &= -ax + c_1y \\ \dot{y} &= c_2x - by \end{aligned}\right\}, \quad ab - c_1c_2 > 0\,;$$

and the solutions are always of the form

$$\begin{aligned} x + \lambda_1 y &= \mathrm{A}\epsilon^{-\mu_1 t} \\ x - \lambda_2 y &= \mathrm{B}\epsilon^{-\mu_2 t} \end{aligned}$$

where the values of λ are given by

$$c_2\lambda^2 - (a-b)\lambda - c_1 = 0\,.$$

We have also $c_2\lambda - a = \mu$, so that the equation for μ is

$$\mu^2 + (a+b)\mu + ab - c_1c_2 = 0\,.$$

The root μ_2, which corresponds to the negative value of λ, is (numerically) greater than μ_1; so that the value of $x - \lambda_2 y$ dies away faster than that of $x + \lambda_1 y$. We may suppose the whole energy to be constantly recruited through the Ps, so that ultimately $x - \lambda_2 y = 0$. This gives the final ratio of the energies of particles, one from each system.]

In the present example, N' vanishes when $e=1$, in which case we have the equations of § 7. For them we had $\lambda_2=1$, which is its least (numerical) value. Hence when $e<1$ we have ultimately

$$x>y;$$

that is, the average energy per Q tends to smaller value than that per P.

11. To work out the consequences of such equations as those of § 9, in the most general form, would lead to details too complex for the ordinary reader. We will therefore take another special case, in order to point out how the results are modified by other simple assumptions of the kind spoken of there.

Let there be three systems of particles such that Ps and Qs are as before, but each Q has an R, which cannot impinge on anything but its special Q. Thus we may suppose each Q in the former arrangement (§ 7) to be made hollow, and to have an R (free) put inside it.

We are not prepared, so far as we have gone, to treat this question if the R's, like the Ps and Qs, have unit coefficient of restitution. For, § 8 above, the R's do not impinge on one another.

But if we suppose the coefficient of restitution of Q and R to be less than unity, and the interior of a Q so nearly equal to the R inside, that between every two collisions of the Q with an external particle there is time for the R inside it to be reduced to relative rest (or what may be treated as such) we may approximate to the ultimate state of things.

The equations in § 6 still hold good for each impact of a P on a Q. But, immediately after the impact, the Q impinges on its R; with the result that, before the Q suffers another collision, v' is reduced to v'', where $(Q+R)v''=Qv'+Rv$, so that instead of the equations in § 7, we have now

$$\varpi\dot{x} = -\frac{4PQ\nu}{(P+Q)^2}\left\{x-\frac{Q}{Q+R}y\right\}$$

$$\rho\dot{y} = \frac{4PQ\nu}{P+Q}\left\{-\frac{y}{Q+R}+\frac{Qx}{(P+Q)(Q+R)}+\frac{PQy}{(P+Q)(Q+R)^2}\right\}$$

$$-\frac{4QR+2R^2}{(Q+R)^2}y\nu'$$

where the last term in the value of y is due to $2\nu'$ sets of impacts between Qs and their Rs, *after* the Qs have collided in pairs.

To get a notion of the nature of this result, suppose as before $\varpi=\rho$, $\nu=\nu'$. Also let $Q=3R$, $P=4R$, then we find by the above methods that ultimately x is nearly the double of y.

If we had unit coefficient of restitution between Q and R, and were to assume Boltzmann's result here, we should have x to y as 1 : 2; because the Qs consist of two separate parts, *each* of which (having the same number of degrees of freedom) would ultimately have the same energy as a P.

12. Without more formidable mathematical processes we cannot well push these investigations further:—but enough has been done to show on what bases Clerk-Maxwell's Theorem really rests; and, at the same time, to show that even were Boltzmann's extension of it rigorously proved, it need not prevent us from accepting the kinetic theory, which has furnished such simple and complete explanations of many puzzling phenomena. It is not at all likely that the particles of any gas (be it even mercury vapour) behave as if their coefficient of restitution were exactly unity. They would probably require, in order to do so, a steady supply of heat; perhaps other things of which we have as yet no knowledge. Among these unknown conditions may be mentioned, so far as the specific heat question is concerned, the nature of the impacts between the particles and the walls of the containing vessel. The law of these impacts, and the mode in which the energy thus received is distributed among the degrees of freedom of a particle, may differ widely from those which regulate the impact of particle on particle.

(*b*) On the Length of the Mean Path among Equal Spheres.

The following investigation has been made as elementary as possible. It will be seen that it leads to a result somewhat different from that usually accepted. The source of the discrepancy is pointed out.

Let s be the diameter of a sphere. It protects a circular area πs^2 in any plane through its centre; in the sense that another sphere, of the same diameter, moving perpendicularly to that plane, will necessarily collide with the first if the line of motion of its centre pass within the circle

Hence if there be a layer of thickness δx, in which quiescent spheres are evenly distributed, at the rate of n_1 per unit volume, and if a group of spheres (whatever their common speed) impinge perpendicularly on the layer, the fraction of them which pass through the layer without collision is

$$1 - n_1 \pi s^2 \delta x.$$

If they impinge obliquely on the layer, we must substitute for δx the thickness of the layer in the direction of their motion.

If the particles in the layer were all moving with a common velocity, we should have to substitute for δx the thickness of the layer in the direction of the *relative* velocity.

So far, all is so obvious as not to require proof. Now suppose v to be the common speed of the impinging spheres, and that they all move perpendicularly to the layer. Also suppose that all spheres in the layer are moving with common speed v_1, but in directions uniformly distributed in space.

Those of them which are moving in directions inclined from β to $\beta + \delta\beta$ to the direction of motion of the impinging particles are, in number per unit volume, $n_1 \sin\beta \, \delta\beta/2$

The virtual thickness of the layer in the direction of relative motion is, so far as these are concerned,

$$\delta x \frac{\sqrt{v^2 + v_1^2 - 2vv_1 \cos\beta}}{v};$$

the term involving the cosine having the negative sign, because the velocity v_1 has to be reversed in finding the relative velocity.

Thus the fraction of the impinging particles which traverses this set without collision is

$$1 - n_1 \pi s^2 \delta x \frac{\sqrt{v^2 + v_1^2 - 2vv_1 \cos\beta}}{2v} \sin\beta\delta\beta.$$

All such expressions, from $\beta = 0$ to $\beta = \pi$, each of them less than unity by an infinitesimal quantity, must be multiplied together to find the fraction of the impinging particles which traverse the layer without collision. The logarithm of this product is

$$-\frac{n_1 \pi s^2 \delta x}{2v} \int_0^\pi \sqrt{v^2 + v_1^2 - 2vv_1 \cos\beta} \sin\beta d\beta.$$

If v be greater than v_1, the value of this is

$$-n_1\pi s^2\delta x\left(1+\frac{v_1^{\,2}}{3v^2}\right);$$

but if v be less than v_1, it is

$$-n_1\pi s^2\delta x\left(\frac{v}{3v_1}+\frac{v_1}{v}\right).$$

These values are equal, as they ought to be, for the case of $v=v_1$.

We must now take account of the distribution of speeds among the particles in the layer. If there be n per unit volume, the number having speeds between v_1 and $v_1+\delta v_1$ is

$$n_1=\frac{4nv_1^{\,2}\delta v_1}{\sqrt{\pi}\,.\,a^3}{}^{-v_1^2/a^2}.$$

Hence the logarithm of the fraction of the whole number of particles, with speed v, which freely traverse the layer is

$$-\frac{4n\pi s^2}{\sqrt{\pi}.\,a^3}\delta x\left(\int_0^v \varepsilon^{-v_1^2/a^2}\left(v_1^{\,2}+\frac{v_1^{\,4}}{3v^2}\right)dv_1+\int_v^\infty \varepsilon^{-v_1^2/a^2}\left(\frac{vv_1}{3}+\frac{v_1^{\,3}}{v}\right)dv_1\right).$$

If we write this, for a moment, as $-e\delta x$, it is clear that

$$\varepsilon^{-e\delta x}$$

represents the fraction of a group of particles with speed v which penetrate unchecked the layer δx, and thus

$$\varepsilon^{-ex}$$

represents the fraction which pass without collision through a distance x. Hence the average depth to which particles with speed v can penetrate without collision is

$$\frac{\int_0^\infty \varepsilon^{-ex}x\,dx}{\int_0^\infty \varepsilon^{-ex}dx}=\frac{1}{e}.$$

The value of e is, of course, a function of v.

If we now suppose the impinging particles to have speeds assorted as they are in the statistically stable distribution, the average free path is to be found by multiplying the free path for each speed by the probability of the particle's having that speed, and adding the results. This gives

$$\frac{4}{\sqrt{\pi}.}\int_0^\infty \frac{{}^2dv}{e}\varepsilon^{-v^2/a^2},$$

or, by the value of *e* above,

$$\frac{1}{n\pi s^2}\int_0^\infty \frac{v^2dv\varepsilon^{-v^2/a^2}}{\int_0^v \varepsilon^{-v_1^2/a^2}\left(v_1^2+\frac{v_1^4}{3v^2}\right)dv_1+\int_v^\infty \varepsilon^{-v_1^2/a^2}\left(\frac{vv_1}{3}+\frac{v_1^3}{v}\right)dv_1},$$

which may be written, after some reductions of an easy kind, in the simpler form

$$\frac{1}{n\pi s^2}\int_0^\infty \frac{4x^4dx}{x+(2x^2+1)\varepsilon^{x^2}\int_0^x \varepsilon^{-x^2}dx}.$$

It is obvious, from what precedes, that if the particles of the medium traversed had been quiescent, the mean path through them (at any speeds) would have been simply the first factor of this, viz.,

$$\frac{1}{n\pi s^2}.$$

Hence the definite integral above, which is, of course, a mere numerical quantity, expresses the ratio in which the mean path is shortened in consequence of the motion of the particles of the medium traversed. By a rough process of quadratures (at intervals of 0·25 from 0 to 3), I find its value to be about

$$0{\cdot}677+$$

but I hope soon to evaluate it more exactly. To check this result I traced by points the curve whose area is expressed by the integral, cut it out in stout tinfoil, and compared its weight with that of a square unit. The result was 0·682, but I had probably allowed too much for the infinitely extended part of the area, which it was very difficult to represent properly by a process of this nature.

Clausius gave, from his point of view (equal speeds, equally distributed in all directions), the factor

$$0 \cdot 75.$$

This can be readily obtained from the above formulæ *before* the introduction of the distribution of speed. It is accurate from the point of view taken by Clausius, but it is inapplicable to the kinetic theory.

Clerk-Maxwell gave the value $\frac{1}{\sqrt{2}}$ or, nearly,

$$0 \cdot 707.$$

But his process is, I think, based on a questionable definition, which has since been adopted by Meyer, Watson, and others, who have written on the Kinetic Theory. It involves the assumption that the mean free path is expressed by

$$\frac{\text{Average speed of a particle}}{\text{Average number of collisions per particle per second.}}$$

But, in order to find either the numerator or the denominator of this fraction, recourse is had to the *ordinary* definition of a mean, that which we have used above.

Those who adopt this deviation, from the ordinary method of finding a mean, must face the question :—Why not adopt another equally plausible deviation, and define the mean free path as

$$(\text{Average time of describing a free path}) \times (\text{average speed}) \,?$$

If n_v be the fraction of the whole particles which have speed v, p_v *their* mean free path ; the definition of the mean free path, which we have adopted as the natural one, gives for its value

$$\Sigma(n_v p_v) .$$

The definition usually adopted gives

$$\frac{\Sigma(n_v v)}{\Sigma\left(n_v \dfrac{v}{p_v}\right)} .$$

That which is suggested above, as an alternative to this last, gives

$$\Sigma\left(n_v \frac{p_v}{v}\right) . \ \Sigma(n_v v) .$$

It gives for the reducing factor the approximate value 0·647; which falls short of 0·677 nearly as much as that, in its turn, falls short of 0·707.

From the point of view here taken, the process usually adopted virtually amounts to assuming that the mean value of a number of fractions is to be found by dividing the mean of the numerators by the mean of the denominators. The reason for the close approximation of the results obtained by these different methods is to be sought in the fact that the great majority of the particles have speeds differing but little from the mean square.

It is usual to express the result of this investigation in the form of the ratio of two fractions;

$$A = \frac{\text{Mean Path}}{\text{Diameter of Particle}},$$

and

$$B = \frac{\text{Volume occupied by the particles}}{\text{Sum of the volumes of the particles}}.$$

The values of B/A are

According to	Clausius	8
„ „	Clerk-Maxwell	$\sqrt{72} = 8{\cdot}48$ nearly
„ „	above reasoning	$\frac{6}{0{\cdot}677} = 8{\cdot}86$ „
„ „	alternative	$\frac{6}{0{\cdot}647} = 9{\cdot}27$.

It may be worth while to remark, in this connection, that the somewhat elaborate process, by which Meyer* obtains the mean number of collisions undergone by a particle in unit of time, can be very much simplified. For, by what is said above, it is easy to see that *ev* represents the average number of collisions which will be undergone, per second, by a particle whose speed is, *and remains*, *v*. Hence, taking account of the distribution of speed among the impinging particles, we have for the average number of collisions per particle, per second, Meyer's expression

$$\frac{16ns^2}{a^6}\int_0^\infty \varepsilon^{-v^2/a^2} v^3 dv \left(\int_0^v \varepsilon^{-v_1^2/a^2}\Bigl((v_1^2 + \frac{v_1^4}{3v^2}\Bigr)dv_1 + \int_v^\infty \varepsilon^{-v_1^2/a^2}\left(\frac{vv_1}{3} + \frac{v_1^3}{v}\right)dv_1\right).$$

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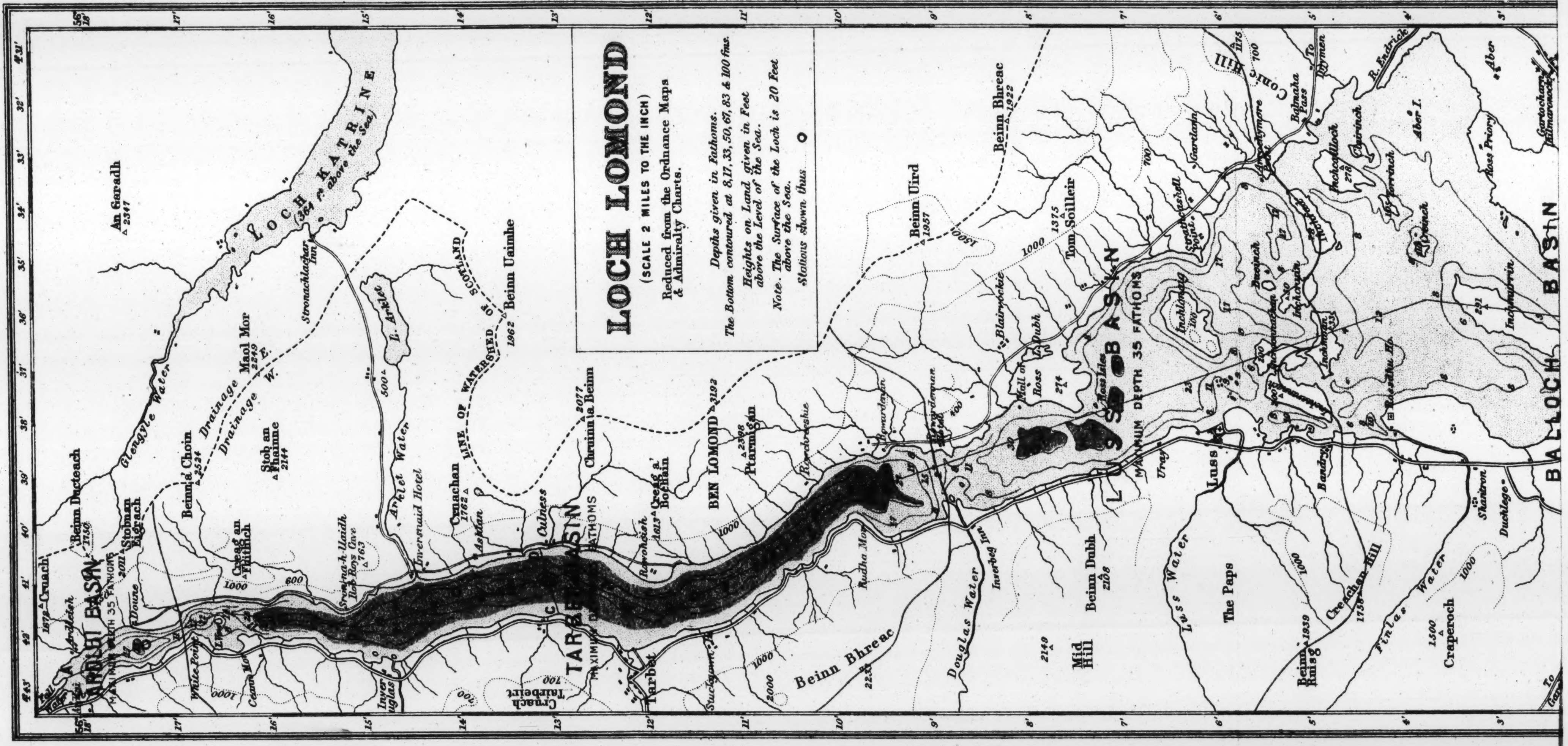
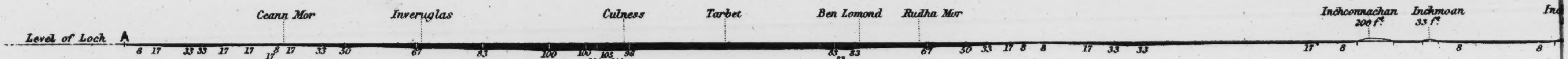
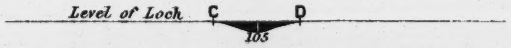
* *Dissertatio de gasorum theoriâ*, 1866. Quoted in his work *Die Kinetische Theorie der Gase*, 1877, p. 294.



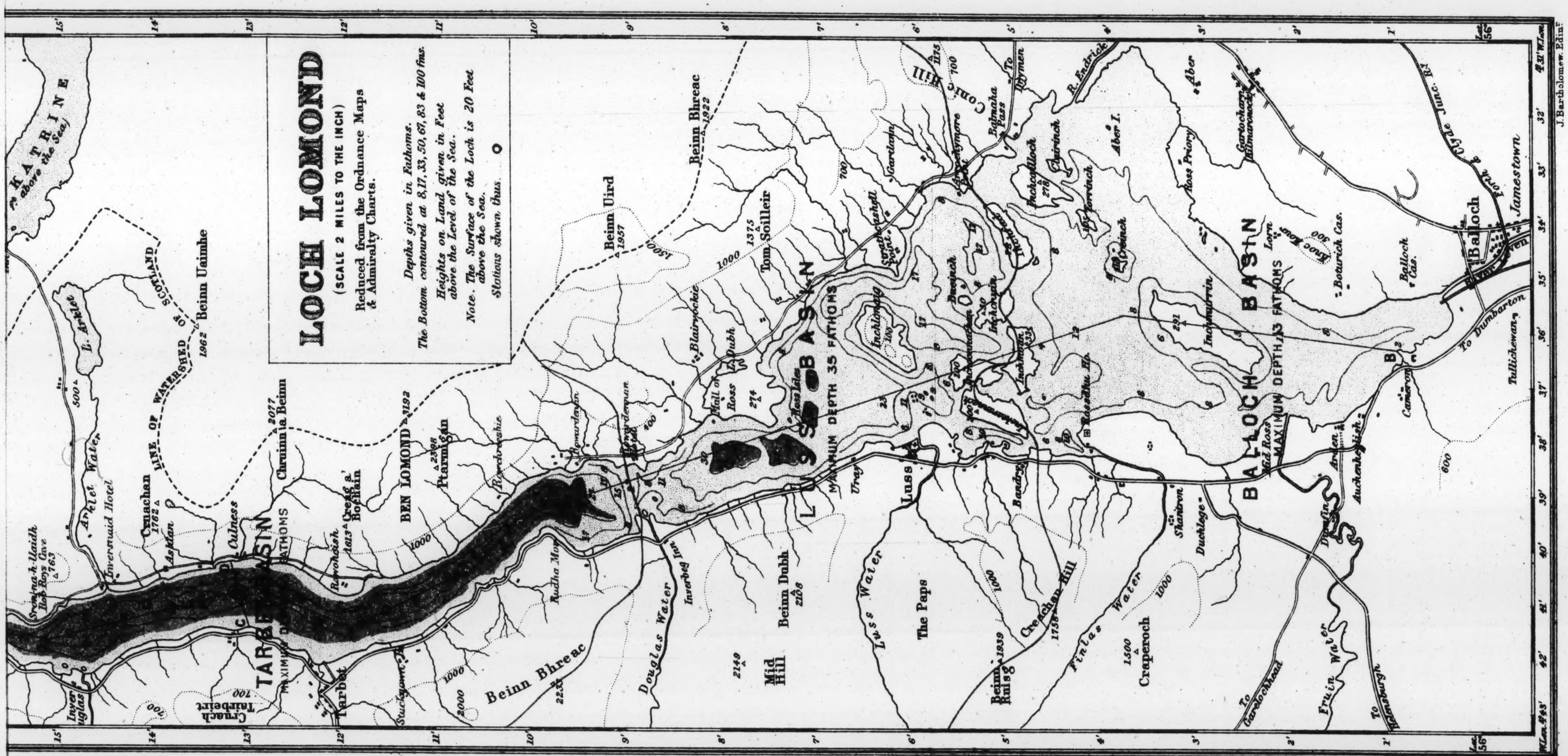


SECTION OF LOCH LOMOND ON THE SCALE OF HALF AN INCH TO THE MILE THROUGH THE LINE A B ON MAP

Depths in Fathoms - Height of Islands in Feet



SECTION ACROSS LOCH LOMOND ON SAME SCALE



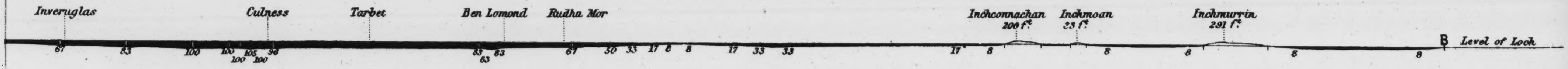
LOCH LOMOND

(SCALE 2 MILES TO THE INCH)

Reduced from the Ordnance Maps & Admiralty Charts.

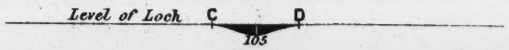
Depths given in Fathoms.
The Bottom contoured at 8, 17, 33, 50, 67, 83 & 100 fms.
Heights on Land given in Feet above the Level of the Sea.
Note. The Surface of the Loch is 20 Feet above the Sea.
Stations shown thus. ○

With the permission of Prof. Jas. Geikie, this map is printed from the plate in his "Great Ice Age."



SECTION OF LOCH LOMOND ON THE SCALE OF HALF AN INCH TO THE MILE THROUGH THE LINE A B ON MAP

Depths in Fathoms - Height of Islands in Feet



SECTION ACROSS LOCH LOMOND ON SAME SCALE

The value of this (which Meyer obtains by expanding in an infinite series, integrating, and summing) may be obtained by noting that while the two parts must, from their meaning, be *equal*, the second part can be integrated at once as it stands.

Or we may change the *order* of integration in the first, and find that it is transformed into the second.

Or we may obtain it by inspection, if we merely transform the double integrals by the assumptions

$$v = r\cos\theta\,,\ v_1 = r\sin\theta\,.$$

Note now that the limits for r are 0 to ∞ in both; but those for θ are 0 to $\pi/4$ in the first, and $\pi/4$ to $\pi/2$ in the second. When this is done we find that the mean number of collisions is $2\sqrt{2\pi}\,ans^2$. Dividing, by this, the mean speed $2a/\sqrt{\pi}$, we have Clerk-Maxwell's value of the mean path $\dfrac{1}{\sqrt{2}\,.\,\pi ns^2}$.

PRIVATE BUSINESS.

The following Candidates were balloted for, and declared duly elected Fellows of the Society:—Dr A. B. Griffiths, F.C.S., Technical College, Manchester; Daniel M. Connan, Esq., Education Department, Cape Town; and David Cunningham, M.Inst.C.E., Dundee.

Monday, 21st December 1885.

PROFESSOR DOUGLAS MACLAGAN, M.D., Vice-President, in the Chair.

The following Communications were read:—

1. On the Distribution of Temperature in Loch Lomond during the Autumn of 1885. By J. Y. Buchanan. (Plate XI.).

In the course of the autumn of this year (1885) I have taken several occasions to determine the distribution of temperature in the water of Loch Lomond. The results of these observations are interesting, as indicating the march of temperature in the different layers at different localities in the lake, and also the gain and loss of heat with the changing seasons.

Loch Lomond is divided naturally into three basins. If the level of the water were reduced by about 8 fathoms, it would form three lakes,—the upper and largest extending from the head of the loch to Rowardennan, the middle one from Rowardennan to the chain of islands stretching from Luss to Balmaha, and the third, and shallowest, from these islands to Balloch. The ridges which separate these basins are covered in the present state of the lake by from 5 to 8 fathoms of water. The lowest, or Balloch basin, is of great extent and comparatively shallow, having a maximum depth of 13 fathoms. The middle, or Luss basin, is also of considerable extent, and has a maximum depth of 35 fathoms. The upper, or Tarbet basin, is long and narrow, and very deep, the maximum depth being 105 fathoms. At the upper end of this basin is a subsidiary one, which I call the Ardlui basin, with a maximum depth of 34 fathoms, and separated from the main basin by a ridge with a probable maximum depth of 17 fathoms.

The general direction of the lake is north and south, so that the prevailing westerly and south-westerly winds blow across it, and, as is always the case in mountainous districts, they are diverted into squalls, which blow sometimes up and sometimes down the lake. At Tarbet there is a deep rift in the mountains separating Loch Lomond from Loch Long, which gives access to the westerly winds to this part of the lake. On the whole, the geographical position of the lake tends to neutralise the effect of the prevailing winds.

Extended temperature observations were made on the following days:—18th August, 5th and 22nd September, 15th October, and 14th November. On the 18th August observations were made only in the Tarbet basin, and only down to a depth of 30 fathoms. On the 5th September observations were made in the Luss basin, at four stations in the Tarbet basin, and at one station in the Ardlui basin. On the 22nd September observations were made in the Luss basin, and at two stations in the Tarbet basin. On the 15th October observations were made at the same stations as on 5th September, omitting Culness; and on 14th November observations were made in the Luss basin and at Inversnaid.

The observations were made with an improved form of protected six's thermometer, having a millimetre scale on the stem and a Fahrenheit's scale on slips at the side. The average length of a degree Fahrenheit was 3 millimetres, and all the thermometers had been carefully and repeatedly compared with each other, and with a Kew corrected standard. The temperatures given are all in terms of the Kew standard. As a rule, the same thermometer has been sent to the same depth. Further, the same sounding line was used on all occasions.

The results are collected in tables, and in some cases they are represented graphically by curves.

If we represent the distribution of temperature graphically by a curve, having depths measured along the horizontal line of abscissæ and temperatures along the ordinates, the winter distribution will be represented by a straight line parallel to the line of abscissæ, such as A. As the spring advances and the meridian altitude of the sun daily increases, the temperature of the surface rises rapidly. The heat received at the surface is, during this season, propagated downwards, chiefly by conduction, which, in water, is a comparatively slow process, hence the temperature of the surface, and of the layers near it, rises much more rapidly than that of those below it, and consequently the curve representing the vertical distribution takes the form B, which, from the bottom to within about 15 fathoms of the surface, preserves its parallelism to the line of abscissæ, but then bends sharply upwards, presenting a well-marked convexity to the origin. This convexity of the curve is the distinctive feature of a *vernal* distribution of temperature. As the summer advances the temperature of the surface no longer increases at the same rate as before, indeed it tends always more and more to become constant. The heat of the surface layers is, however, always being propagated downwards by conduction, and when the temperature of the surface layer has become nearly constant, it follows that, at some depth a little below the surface, the temperature will be rising more quickly than in the layers above, and this produces a slight bulge in the curve C, representing the distribution. This part of the curve presents a concavity to the origin which, combined with the pronounced convexity below and the less marked convexity above, produces the typical *summer* distribution. When the autumn has set in, and the surface temperature falls from day to day, heat is still being propagated downwards by conduction and convection, the curve takes the typical autumnal form D, consisting of a horizontal piece near the surface united to another horizontal piece near the bottom by the summer concavity and the vernal convexity. Hence in the autumn the waters of a deep lake are exposed to all the different conditions of the four seasons of the year. In the deeper layers heat is propagated down-

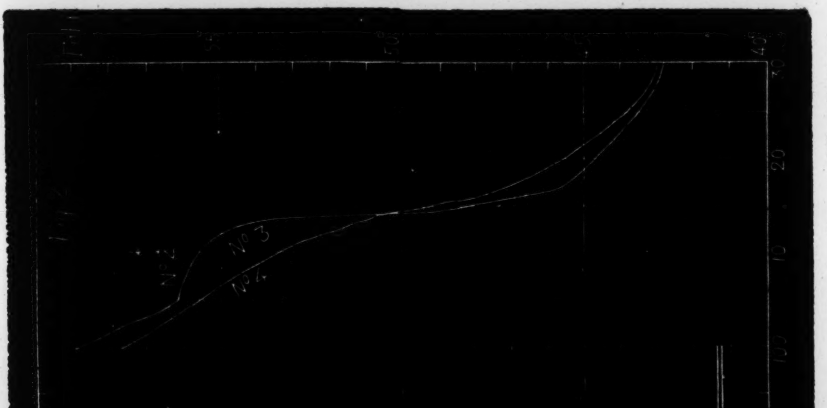


Fig. 2.—Curves of Temperature, 18th August 1885.

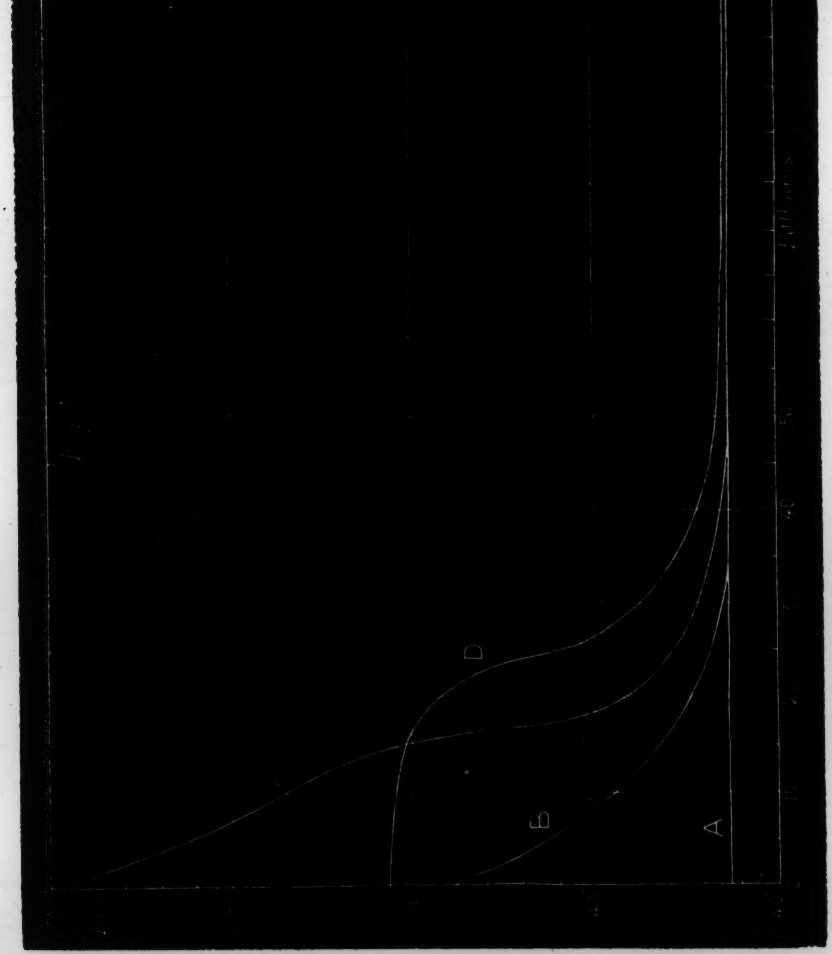


Fig. 1.—Typical Curves of Distribution of Temperature in a Scottish Lake 100 fathoms deep. A, in winter; B, in spring; C, in summer; and D, in autumn.

wards most rapidly in the first half of the month of October. As the winter progresses heat leaves the water so rapidly by the surface that conduction downwards is checked and the deeper waters derive but very slight benefit from the summer heat at the surface.

Observations on 18*th August* 1885.—These observations were confined to the Tarbet basin, and were only carried to a depth of 30 fathoms. The weather was perfect for sounding operations, being quite calm, so that the steam launch, which was used for the work, remained in position without trouble. The sun was very powerful all day, and its heating effect may be judged from the fact that at 10 A.M. the surface water in the channel off Camstradden was 58°·9, and at 5.30 P.M. in the same position it was 62°·6 F., indicating a rise of 3°·7 in the course of the day. As an assistance to finding the positions of the stations, I give the distance in nautical miles in a straight line from Balloch pier; as a rule, they were made in the deepest part of the loch in the locality.

TABLE I.—*Observations in Loch Lomond*, 15*th October* 1885.

Locality,		Rowardennan.	Rowcreeshie.	Culness.	Rob Roy's Cave.	Range of Temperature at the same depth.
Miles from Balloch Pier,		9	10	12¾	15	
Depth at Station,		25	70	100	60	
Hour of day, .		noon.	3.30 P.M.	1 P.M.	2 P.M.	
No. of Station, .		1	2	3	4	
Depth. Fathoms.	No. of Thermometer.	Temperature (Fahr.).				
0	...	57°·5	58°·7	58°·4	57°·7	1°·2
5	21	56·75	56·2	56·3	55·5	1·25
10	9	56·0	55·75	54·3	53·45	2·55
15	23	49·45	48·85	47·95	50·0	2·05
20	47	44·5	45·0	45·2	45·8	1·3
25	79	43·35	43·85	44·0	43·8	0·65
30	80	...	42·95	43·35	43·0	0·4
0 to 30 mean,		...	50·90	49·74	49·8	0·35
Steepest Gradient,	Degs. per fm.	1·31	1·38	1·26	0·84	...
	Depth,	13	13	12	17	...

The distribution in series Nos. 2, 3, and 4 is graphically represented in fig. 2, and it will be seen that the curves have a marked summer character. The steepest gradients are between 10 and 15 fathoms. At Culness (No. 3) the fall of temperature is 6°·9 in this interval, or 1°·38 per fathom. The least steep gradient is on the northmost station, No. 4, indicating greater mixture of the layers of water towards the head of the loch. The curves show also, in a remarkable manner, the difference in temperature of the water at the same depth in different localities. The greatest difference is found at 10 fathoms, at which depth the temperature at Rowardennan exceeds that at Rob Roy's Cave by 2°·55. Although the

TABLE II.—*Observations in Loch Lomond, 5th September* 1885.

Name of Basin,		Luss.	Tarbet.				Ardlui.
Locality,		Ross Mill.	Rowardennan.	Stuckgowan.	Culness.	Inversnaid.	Doune Farm.
Miles from Balloch Pier, .		7½	9	11	12¾	14	17
Depth at Station,		33	37	87	100	100	34
Hour of day, .		11 A.M.	Noon.	1 P.M.	2 P.M.	3 P.M.	4.20 P.M.
No. of Station, .		5	6	7	8	9	10
Depth. Fathoms.	No. of Thermometer.	Temperature (Fahr.).					
0	...	56°·2	56°·5	56°·4	...	56°·0	56°·5
5	9	55·9	55·1	55·3	...	55·8	56·3
10	80	55·75	54·8	54·8	...	55·75	55·75
15	79	49·1	51·25	49·0	...	49·8	48·1
20	47	48·3	45·6	45·8	...	45·6	46·5
30	21	47·0	43·2	43·5	...	43·2	45·25
35	47	...	...	...	42·4	42·7	...
40	79	...	...	42·5	...	...	...
45	79	...	...	...	42·15	42·2	...
50	9	...	...	42·05	...	...	...
65	9	...	...	...	41·85	41·8	...
70	80	...	...	42·0	...	...	...
80	47	...	...	41·75	...	...	...
85	80	...	...	...	41·8	41·7	...
Btm. 87	21	...	...	41·8	...	...	...
Btm. 100	21	...	...	...	41·8	41·8	...
0 to 30 mean,		53·93	51·23	50·87	...	51·32	51·57
Steepest gradient,	Degs. per fathom,	1·33	1·13	1·16	...	1·19	1·53
	Depth,	13	16	13	...	15	13

distribution varies at the different stations, there is little difference in the mean temperature of the 30 fathoms, it is higher at Rowcreeshie than farther north. From the surface to 10 fathoms the highest temperatures are at the lower end of the basin, from 15 to 30 they are nearer the upper end. At 15 fathoms the temperature at Culness is lower than either north or south of it.

5th September 1885.—All day the weather was most favourable for experimenting, except perhaps at Culness, when it threatened for a few minutes to blow and rain. Otherwise it was almost quite calm, with overcast sky, so that there was no overheating by the sun or cooling by the wind.

Positions—No. 5.—Outer Ross Island bears S. 27° E. (true), distant 0·74′.

No. 6.—Rowardennan Lodge bears N. 102° E. (true), distant 0·43′.

No. 7.—Stuckgowan Lodge bears S. 67° W. (true), distant 0·32′ to 0·37′.

No. 8.—Tarbet Pier bears S. 43° W. (true), distant 1·3′.

No. 9.—Inversnaid Inn bears N. 34° E. (true), distant 0·7′.

No. 10.—Stuckindroir House bears N. 60° W. (true), distant 0·38′.

All the places mentioned in this paper are to be found in the Admiralty Chart of the lake.

Owing to the overcast state of the sky, there is little variation in the temperature of the surface. Below the surface there is again considerable variation, but the maximum range, 2°·25 at 15 fathoms, is less than was observed on 18th August. The character of the distribution is distinctly autumnal. On 18th August the observations were confined to the Tarbet basin and were limited to 30 fathoms; to-day they extend to the three deep basins of the loch. The Ardlui and the Luss basins resemble each other in that their maximum depth is about the same—34 fathoms; but the Ardlui basin is separated from the Tarbet one by a ridge of probably 17 fathoms, while the ridge shutting off the Luss basin has a maximum depth of only 8 fathoms, cold deep water is thus enabled to penetrate from the Tarbet basin into the Ardlui basin, but not into the Luss basin. Further, the Ardlui basin receives, for its size, a much greater supply of the affluent waters from the land, so that its waters in winter are probably colder than those of the lake lower down.

In accordance with the autumnal character of the distribution, the temperature of the first 10 fathoms approaches uniformity at all the stations. It is highest at Ardlui, being 56°·2, and lowest at Rowardennan, being 55°·1. The steepest gradients are all between 10 and 20 fathoms. They are steeper in the shallow basins than in the deep ones; in the Ardlui basin the average gradient is 1°·53 per fathom between 10 and 15 fathoms.

On 7th September the Inversnaid station was revisited, and the temperatures on the gradients accurately ascertained by sending thermometers to every fathom, from 13 to 17 inclusive, with the following result:—

Observations at Inversnaid, 7th September 1885.

Depth.	No. of Thermometers.	Temperature degs. Fahr.	Gradient, deg. per fathm.
13	21	52°·0	...
14	47	51·25	0·75
15	79	49·8	1·45
16	9	48·8	1·0
17	80	47·25	1·55

The mean gradient in these four fathoms of water is 1°·19 per fathom, the maximum is 1°·55 between 16 and 17 fathoms. It is therefore probable that the actual maximum gradient in the Ardlui basin may be as much as 2° per fathom.

Owing to a mistake, 105 fathoms of line were paid out at Culness and Inversnaid instead of 100, which accounts for the irregular intervals between the thermometers at the deeper depths. From 30 to 70 fathoms the temperature of the water is slightly higher at Stuckgowan than at Inversnaid. From 70 fathoms to the bottom the water at the three deep stations is sensibly uniform, namely, 41°·8. On 7th September the three thermometers, Nos. 47, 79, and 80, were sent down together to 60 fathoms at Inversnaid, and their corrected temperatures were 41°·9, 41°·9, and 41°·85. There is, therefore, a fall of 0·1° between 60 and 100 fathoms at this season of the year. The same thermometers were also sent to 30 fathoms in the Luss basin, and their corrected readings were 47°·0, 47°·0, and 47°·0.

TABLE III.—*Observations in Loch Lomond, 22nd September* 1885.

Name of Basin, . . .		Luss.	Tarbet.	
Locality, ...		Ross Mill.	Rowandennan.	Inversnaid.
Miles from Balloch Pier, . .		7½	9	14
Depth at Station, . . .		33	37	100
Hour of the day, . . .		1.30 P.M.	2.30 P.M.	4 P.M.
No. of Station, ...		11	12	13
Depth. Fathoms.	No. of Thermometer.	Temperature (Fahr.).		
0	...	53°·6	53°·6	53°·7
5	9	53·5	53·35	53·7
10	80	53·45	53·4	53·65
15	79	53·4	52·3	52·25
20	47	48·8	45·1	47·2
30	21	47·55	42·9	43·5
35	47	...	...	42·6
45	79	...	...	42·25
65	9	...	...	42·0
85	80	...	...	41·8
Btm. 100	21	...	...	41·8
0 to 30 mean, . . .		51·94	50·49	51·08
Steepest gradient,	Degs. pr. fath.,	0·9	1·44	1·01
	Depth, .	18	17	18

22nd September 1885.—The weather was very stormy, and had been so for a fortnight, with much rain, so that the level of the lake was very high. Wind fresh from the south-west. The weather was so squally that it was difficult to keep station. At the Ross Mill station I kept the launch head to wind with a couple of oars out, but it was not very successful. At the other two stations I kept her stern to wind, with a steer-oar out over the bow and occasionally driving the engine astern. At Inversnaid the wind was so strong that I was able to keep the engine going continually dead slow astern, and kept station well.

The character of the distribution is pronouncedly autumnal, cooling at the surface is going on rapidly while heat is being propagated into the lower layers. The surface layer of approximately constant temperature is now about 15 fathoms thick at all the stations, and the steepest gradients are found between 15 and 20 fathoms, the maximum being 1°·44 per fathom at Rowardennan.

The bottom temperature has not sensibly altered at Inversnaid since 5th September. In the Tarbet basin the greatest difference of temperature at the same depth is 2°·1 at 20 fathoms.

TABLE IV.—*Observations in Loch Lomond, 15th October* 1885.

Name of Basin, . .		Luss.	Tarbet.			Ardlui.
Locality, . .		Ross Mill.	Rowardennan.	Stuckgowan.	Inversnaid.	Doune Farm.
Miles from Balloch Pier,		7½	9	11	14	17
Depth at Station, . .		33	37	91	100	34
Hour of the day, . .		11 A.M.	Noon	1 P.M. 4.30 P.M.	1.30 P.M. 3.30 P.M.	2.30 P.M.
No. of Station, . .		11	12	13	14	15
Depth. Fathoms.	No. of Thermometer.	Temperature (Fahr.).				
0	...	49°·4	49°·1	49°·1	48°·7	47°·8
5	9	49·2	49·0	49·05	48·4	46·7
10	80	49·05	49·05	49·0	48·2	46·3
15	79	49·0	49·0	49·0	48·1	46·3
20	47	49·1	48·95	48·8	47·8	46·2
30	21	48·9	42·44	42·9	45·3	45·9
35	47	...	...	...	44·2	...
40	79	...	...	42·4	...	...
45	79	...	...	...	42·8	...
50	9	...	...	42·2	...	...
65	9	...	...	...	42·2	...
70	80	...	...	42·1	...	...
80	47	...	...	42·0	...	...
85	80	...	...	...	42·0	...
90	21	...	...	42·0	...	...
Btm. 100	21	...	...	...	42·0	...
0 to 30 mean, . .		49·0	48·37	48·37	47·9	46·47
0 to 20 " . .		49·12	49·04	49·00	48·24	46·58

15th October 1885.—A very fine day, with moderate north-easterly wind. For three days a strong northerly wind had been blowing with dry weather, and for a fortnight before there had been a succession of cyclonic gales, with much rain, so that at the beginning of the week the level of the lake was higher than I have ever seen it, and quite 3 feet above its usual summer level. The operations were all successfully carried out, the launch being kept stern to wind.

Amongst the salient features of the distribution of temperature at this date may be mentioned the rapid cooling which has taken place in the interval since 22nd September, the mean temperature of the first 30 fathoms having fallen about 3° F. at Ross Mill and Inversnaid. It will be seen that in the Luss basin the distribution has become nearly quite uniform from surface to bottom, and at Ardlui the water is rapidly approaching the same condition. The temperature of the water from the surface to 20 fathoms is practically uniform. At Ross Mill, in the Luss basin, and at Rowardennan and Stuckgowan, in the Tarbet basin, it is much alike, namely, 49°·12, 49°·04, and 49°. At Inversnaid it has fallen to 48°·24, and at Ardlui it is as low at 46°·58. The depth of water at Ross Mill and at Ardlui is nearly identical (34 fathoms), but the Luss basin is large, broad, and situated near the outflow of the lake; while the Ardlui basin is small and confined, and situated close to the head of the loch, so that it might almost be taken as forming part of the embouchure of the river Falloch. On the 5th September the temperature of the water down to 15 fathoms was nearly alike in the two basins, but below 15 fathoms it was much colder at Ardlui than at Ross Mill, the difference being 1°·75 at 30 fathoms.

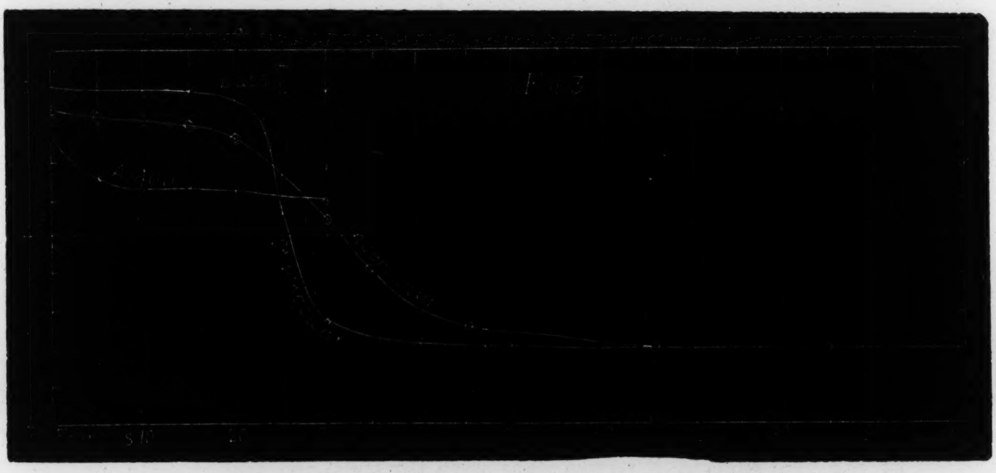


FIG. 3.—Curves of Temperature, 15th October 1885.

In the following table the mean temperature of the water found between the surface and 20 fathoms, the surface and 30 fathoms, and between 20 and 30 fathoms, are given for Luss and Ardlui as observed on 5th September and 15th October:—

TABLE V.—*Comparative Table of the Mean Temperature of certain intervals in the waters of the Ardlui and the Luss Basins on* 5*th September and* 15*th October* 1885.

Interval, . . .	Surf. to 20 fms.		Surf. to 30 fms.		20 to 30 fms.	
Date, . . .	Sept. 5	Oct. 15	Sept. 5	Oct. 15	Sept. 5	Oct. 15
Mean Temp. at Luss,	53°·24	49°·12	51°·37	49°·00	47°·65	49°·00
Mean Temp. at Ardlui,	52°·90	46°·58	50°·33	46°·40	45°·85	46°·05
Difference, . . .	0°·34	2°·54	1°·04	2°·60	1°·80	2°·95

A comparison of these data brings out very clearly the difference in conditions obtaining in the two basins notwithstanding their likeness in depth. At Ardlui the predominating influence is that of the important tributary, the Falloch, which influences the temperature of the lake water in its neighbourhood most, while its temperature is lower than that of the lake. On 16th October the temperature of the water of the Douglas was 44°·6 F., while that of the surface of the lake in its neighbourhood was 49°·0. On 13th November the temperature of the stream at Inversnaid was 40°, and that of the lake surface 46°. From its rise and course the Falloch is more likely to be colder than warmer than these streams, so that even in October it must have begun to spread its cooling influence over the lower waters of the Ardlui basin. When the water of the stream is warmer than that of the lake surface, it passes away with the drainage, and imparts as much of its heat to the atmosphere above it as to the water below. When its temperature is lower than that of the lake surface, and in all probability it is so for more than half the year, it sinks into the body of the lake, and imparts its cold entirely to its deeper waters. It is obvious then that during the time that it is colder than the lake, the water of the Falloch must produce a much greater effect on it than during the opposite season; hence the position of the Ardlui station, with respect to the principal tributary of the lake, renders it natural to expect that its waters would be colder than they are found to be in the Luss basin, which, from its size and position, is comparatively exempt from the direct influence of tributary waters.

Both in September and in October the temperature of all these bodies of water is lower at Ardlui than at Luss, but the contrast is much greater in the colder than in the hotter month.

In the Tarbet basin a salient feature is the greater mixture of waters at Inversnaid than at either Rowardennan or Stuckgowan, the curves at the latter localities being much steeper than at Inversnaid. The same phenomenon was observed on the 18th August. But, perhaps, the principal feature in the Tarbet basin is that both at Inversnaid and at Stuckgowan the water at and near the bottom has risen in temperature by 0°·2 F., or from 41°·8 to 42°·0 since the 22nd September. As the temperature of the bottom water at these localities was 41°·8 on the 5th September, it is probable that it had been so during the summer, and it is only by the end of September or beginning of October that the summer heat begins to have any effect on the water near the bottom. The steepest gradients are at Rowardennan and Stuckgowan between 20 and 30 fathoms. With a view of further investigating this body of water, I returned on 16th October to the Rowardennan and Stuckgowan stations, and took the temperature at 20, 22½, 25, 27½, and 30 fathoms, using, at 20 and 30 fathoms, the same thermometers as had been used the day before. As the temperatures at 20 and 30 fathoms were found very different from those observed the day before, the observations in the Rowardennan locality were repeated close to the east side of the loch, the usual station being nearer the west side. The following table gives the temperatures observed on 16th October and also the corresponding ones of 15th October.

TABLE VI.—*Temperatures on Steepest Gradient at Rowardennan and Stuckgowan, 16th October* 1885.

Locality, .		Rowardennan.			Stuckgowan.	
		West Side.		East Side.	West Side.	
Date, . .		Oct. 15.	Oct. 16.	Oct. 16.	Oct. 16.	Oct. 16.
Depth.	No. of Thermometers.	Temperature (Fahr.).				
0	...	49°·1	49°·0	48°·9	49°·1	49°·0
20	47	48·95	47·8	47·6	48·8	48·4
22½	9	...	47·4	46·4	...	47·3
25	79	...	46·55	46·0	...	47·0
27½	80	...	45·3	45·5	...	46·75
30	21	42·45	45·2	45·25	42·9	44·5

These results go to accentuate the fact borne out by all the observations quoted in this paper, and by all the observations which I have been able to make in other lakes, namely, that at any one date, especially during the warm half of the year, the isothermal surfaces, even at depths where there is no rapid change of temperature, are not planes, but have many curvatures and unevennesses. These unevennesses are particularly accentuated in the region of most rapid change of temperatures or on the steepest gradient of the temperature curve. Here the movements of the thermometer a few yards in a horizontal direction may place it in water of very different temperature. On both days a fresh breeze was blowing, and, though it was possible to keep station very satisfactorily from a nautical point of view, the station kept was an average one—that is, instead of being a point, it was an area, and an area perhaps 20 to 30 yards long by 10 to 20 yards wide. The investigation of the body of water having the steepest temperature gradient is very interesting, but it should be attempted only under the most favourable circumstances—either the weather should be perfectly calm, or the boat should be anchored. In future work the minute delineation of the steepest part of the temperature gradient should have an important place.

TABLE VII.—*Observations in Loch Lomond, 14th November* 1885.

Name of Basin,		Luss.	Tarbet.
Locality,		Ross Island.	Inversnaid.
Miles from Balloch Pier,		7	14
Depth at Station,		34	100
Hour of day,		3 P.M.	1 P.M.
No. of Station,		16	17
Depth. Fathoms.	No. of Thermometer.	Temperature (Fahr.).	
0		46°·6	46°·0
10	Negretti's overturning Thermometers.	46·3	45·8
20		46·3	45·8
30		46·3	44·3
30		...	44·2
40		...	42·3
50		...	42·2
65	9	...	42·15
84	80	...	42·1
100	21	...	42·1

14*th November.*—It had been raining all night, but cleared when I arrived at Balloch. I was accompanied by Mr Morrison, from the Scottish Marine Station at Granton, who brought with him three overturning thermometers. A very heavy snow squall occurred on the way up the loch, but it cleared off before we arrived at Inversnaid. While at Inversnaid the weather was very favourable, and we had no difficulty in getting this series of observations. The temperatures at 65, 85, and 100 fathoms were taken with my thermometers, the others were taken with the overturning thermometers of the Negretti type. In the afternoon a series of temperatures was taken with the overturning thermometers in the Luss basin west of the Ross Islands.

The salient feature at both stations is the great cooling which has taken place since 15th October. The whole body of water in the Luss basin has been cooled 2½°. The temperature of the bottom water at Inversnaid has risen 0°·1, and it has probably reached its maximum.

Having described and discussed the observations made at the different stations at the same date, it will be useful to consider some of the stations with respect to the variation in the distribution of temperature with changing season, and for this purpose it will be convenient to take two typical stations, namely, that at Ross Mill in the Luss basin, which represents a shallow lake, and that at Inversnaid, representing a deep one.

The Luss Basin.—Observations were made in this basin on 5th September, 22nd September, 15th October, and 14th November. Already on the 5th September the curve has almost lost its summer feature, and is becoming pronouncedly autumnal. On 15th October it has assumed the winter form, and between that date and 14th November the nearly uniform temperature of the water has fallen about 2°·6 F. The surface temperature falls from 55°·2 on 5th September to 53°·6 on 22nd September, to 49°·4 on 15th October, and 46°·6 on 14th November. The autumnal zone of nearly uniform temperature in the surface layer extends on 6th September to 15 fathoms, and on 15th October to the bottom. Therefore, until some date between the 22nd September and 15th October the upper stratum of water is being cooled on both sides—that is, it loses heat by radiation and convection upwards into the atmosphere, and by conduction and convection downward into the lower strata of the water. By the 15th October the loss downwards has ceased, and the heat lost between that date and 15th November has escaped by the surface.

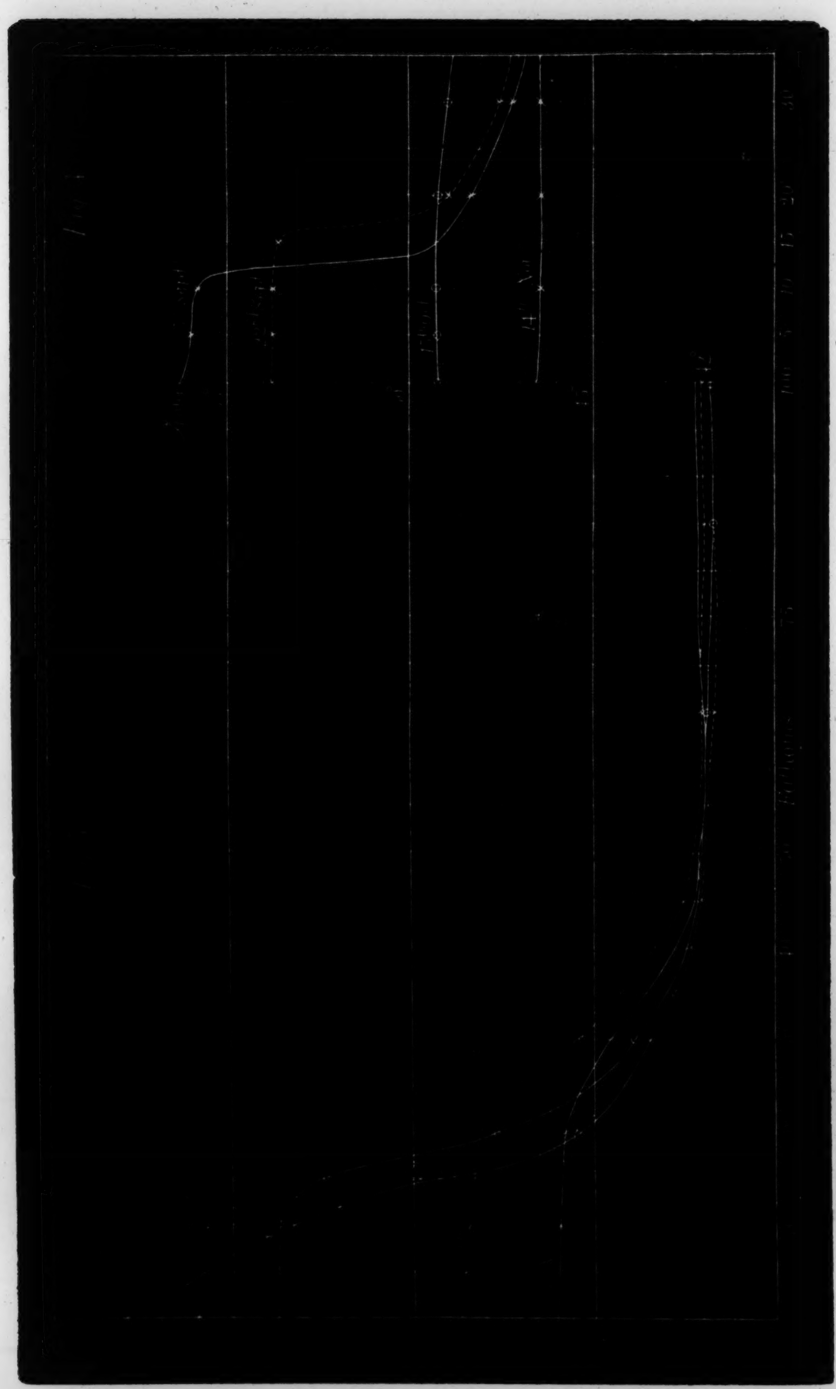


Fig. 4.—Curves of Temperature in Luss Basin, 1885.

Fig. 5.—Curves of Temperature in Tarbet Basin Inversnaid, 1885

TABLE VIII.—*Collected Observations in Luss Basin*, 1885.

Date,	Sept. 5.	Sept. 22.	Oct. 15.	Nov. 14.
Depth.	Temperature (Fahr.).			
0	56°·2	53°·6	49°·4	46°·6
5	55·9	53·5	49·2	...
10	55·75	53·45	49·05	46·3
15	49·1	53·4	49·0	...
20	48·3	48·8	49·1	46·3
30	47·0	47·55	48·9	46·3

Taking the depth at the Ross Mill station as 35 fathoms, we have the following values for the mean temperature of the whole of the water :—

Date, . .	5th Sept.	22nd Sept.	15th Oct.	14th Nov.
Mean temperature,	50°·71	50°·71	49°·03	46°·3

The mean temperatures on the 5th and 22nd September are identical, hence the epoch of maximum heat in the water must fall between these two dates, and as cooling goes on more rapidly than heating, it will fall nearer the 22nd than the 5th, and may with safety be said to occur in the third week of September. The curves of the 5th and 22nd September intersect at a depth of 13·5 fathoms, hence the temperature at 13·5 fathoms was the same on both these dates. But on the earlier one the temperature of the water at that depth was rising, whereas at the later one it is falling, therefore it must have attained a maximum some time between these dates, and no doubt in the third week of September. The curves of 22nd September and 15th October intersect at 19·5 fathoms; here the temperature of the water at both dates was 49°·0. The maximum temperature in this layer must have been attained towards the end of the first week of October. Similarly, from the intersection of the curves of 5th September and 15th October, we should infer that the maximum temperature at 16 fathoms occurs in the last days of September. It is probable that temperature at the bottom (35 fathoms) was about its maximum on 15th October.

TABLE IX.—*Gain and Loss of Heat above and below the Depth corresponding to the intersections of each pair of Curves for the Station in Luss Basin* (1885).

Dates.	Sept. 5.	Sept. 22.	Sept. 5.	Oct. 15.	Sept. 22.	Oct. 15.	Oct. 15.	Nov. 14.
Interval, days, .	17		40		23		30	
Depth of intersection, fathoms,	13·5		16		19·5		none.	
Mean temp. of water above intersection,	55°·7	53°·45	55°·0	49°·2	52°·89	49°·1	49°·03	46°·30
Heat in fathom-degrees in water above intersection, . .	780	748	880	787	1031	957	1716	1620
Loss of heat in interval, . .	32		93		74		96	
Mean temp. of water below intersection, .	46°·28	47°·85	47°·10	48°·90	48°·07	48°·95		
Heat in fathom-degrees in water below intersection, . .	995	1028	895	929	745	759		
Gain of heat in interval, . .	33		22		14			
Percentage of heat passed downwards, .	100		24		19		0	
Heat passed into the air per day,	0		1·78		2·61		3·2	
Heat passed down to water per day, . .	1·94		0·55		0·61		0	
Mean temp. of the 35 fathoms,	50°·71	59°·71	...	49°·03	...	...	...	46°·30

The points of intersection of the curves indicate depths where the temperature of the water was found to be the same on both days. Although the temperature of the water at this depth has risen and fallen in the interval, it has returned to the same thermal state at the end of the interval as at the beginning. In Table IX. the distribution of heat in the water above and below these points is indicated for the different pairs of dates. For convenience' sake, heat units are expressed in *fathom-degrees.**

From the table we see that between the 5th and 22nd September the water as a whole has lost as much heat as it has gained, so that all the heat that the layer above the intersection at 13·5 fathoms has lost appears in the deeper water below the intersection. On an average over the whole period, no heat has been dissipated to the atmosphere. This, of course, is only true on an average; for during the first portion of the period the water has been receiving heat, and during the second portion it has been dissipating it at the surface. During this interval the heat passed downwards to the deeper layers has been at the rate of 1·94 fathom-degrees per day. Between the 22nd September and 15th October the amount of heat transmitted downwards has been 14 per cent. of the total loss above the intersection. The heat dissipated per day to the atmosphere is 2·61 units, and that passed downwards 0·61 unit. The curve of 14th November does not cut that of 15th October, and the loss of heat is at the rate of 3·2 units per day, all of which has gone out into the atmosphere. On the 23rd November 1876 I found the water of the Luss basin to have a uniform temperature 47°·8 from surface to bottom. In the year 1876, therefore, the water had a temperature 1°·5 higher on the 23rd November than it has in 1885 on 14th November, or nine days earlier. For the same date the water is this year at least 2°·5 colder than it was in 1876, and with this rapid and severe autumnal cooling it will require no very hard or long-continued frost to freeze the great shallow Balloch basin of the loch. In fact, it will be found that in years when Loch Lomond has been frozen there has always been an exceptionally cold autumn. The prolonged action of the low temperature of a cold autumn prepares the water for any severe frost which may occur in the long nights about Christmas. Under its influence the water rapidly freezes. When the Balloch basin was frozen in the winter of 1878–79, I took the temperature of the water beneath the ice in several places, the deepest water being 11 fathoms. The temperature of the whole of the water was under 35° F., and varied from 32° in contact with the ice to 34°·5 at the bottom. In the long clear nights of a severe winter the temperature of the atmosphere may often be 30° below that of the surface of the water, so that both by radiation and convection cooling goes on with great rapidity, notwithstanding the fact that water expands in being cooled below 39° F.; and it is remarked by people dwelling by the side of the lake that the moment ice begins to freeze it spreads with great rapidity, so that the whole basin is usually frozen over in a night, and as soon as a skin of ice has been formed it very rapidly becomes thick enough to bear.

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* The fathom-degree is one fathom heated 1° F. If the fathom has a sectional area such that the volume of water weighs one pound, then the fathom-degree is the same as the ordinary heat unit.

Epochs of Maximum Temperature.—From the curves and their intersections we see that the maximum temperature of the surface water occurred before the 5th September. At 13·5 fathoms it occurred some time between the 5th and 22nd September, and as cooling goes on faster than heating, the date of maximum temperature at this depth will be nearer the 22nd than the 5th September, probably about the 15th. Similarly at 16 fathoms it occurs between the 5th September and 15th October, and probably about 30th September. At 19·5 fathoms the maximum temperature occurs about 6th October, and in the second week of October (in 1885) all water from 20 fathoms to the bottom attained and passed its maximum temperature. The maximum temperature of the surface occurs about the middle of August, and that of the bottom in 35 fathoms about the middle of October.

Inversnaid Station.—The observations at this station at the different dates have, in many respects, greater interest than those made in the Luss basin. The depth here is 100 fathoms, and is so great that for a thickness of 40 or 50 fathoms above the bottom the change of temperature during the course of the season amounts only to a fraction of a degree, and is so slight as to elude detection, except by using very delicate thermometers. The observations made at Inversnaid are collected in Table X., and the results are expressed graphically in the curves (fig. 5). On 18th August no observations were made exactly on the Inversnaid station, but observations were made at two neighbouring stations, Rob Roy's Cave, about a mile north, and Culness, about a mile south of it. The curve has been drawn from the means of the temperatures observed at these two stations.

TABLE X.—*Inversnaid Station, Collected Observations.*

Date.	Sept. 5.	Sept. 22.	Oct. 15.	Nov. 14.
Depth.	Temperature (Fahr.).			
0	56°·0	53°·7	48°·7	46°·0
5	55·8	53·7	48·4	...
10	55·75	53·65	48·2	45·8
15	49·8	52·25	48·1	...
20	45·6	47·2	47·8	45·8
30	43·2	43·5	45·3	44·3
35	42·7	42·6	44·2	...
40	...	...	...	42·3
45	42·2	42·25	42·8	...
50	...	...	...	42·2
65	41·8	42·0	42·2	42-15
85	41·7	41·3	42·0	42·1
100	41·8	41·8	42·0	42·1

Table XI. gives the analysis of the results of the observations at Inversnaid, and is a form of *heat account* for the period—that is, it gives the receipt and expenditure of heat during the various intervals with reference to the points of intersection of the curves, where receipt and expenditure exactly balance each other over the period under consideration. Between the 18th August and the 5th September, an interval of 18 days, during which the temperature of the layer immediately at the surface had begun to fall, five times as much heat was conveyed downwards as was dissipated from the surface. Between the dates 5th September and 22nd September the two quantities almost exactly balance one another. Between 22nd September and 15th October the amount transmitted downwards is only half what leaves the surface; and between the 15th October and 14th November the amount transmitted downwards is quite insignificant—not more than 4 per cent. of what escapes to the air. The activity in the heat exchange has been greatest between 22nd September and 15th October when it has been dissipated at the surface at the rate of 3·74 fathom-degrees, and conveyed downwards at the rate of 2 fathom-degrees per day. The crest of the heat wave passes from surface to bottom in about three months, the height of it decreasing very rapidly as the depth increases.

TABLE XI.—*Gain and Loss of Heat above and below the Depths corresponding to the intersections of each pair of Curves for the Station at Inversnaid,* 1885.

Dates.	Aug. 18.	Sept. 5.	Sept. 5.	Sept. 22.	Sept. 22.	Oct. 15.	Oct. 15.	Nov. 14.
Interval (days), . .	18		17		23		30	
Depth of intersection (fathoms), . .	5		14		19·5		65	
Mean temperature of water above intersection, . . .	57°·0	55°·9	55°·62	53°·62	52°·65	48°·24	45°·21	43°·93
Heat in ditto (fathom-degrees), . . .	285·0	279·5	779	751	1027	941	2939	2856
Loss of heat in interval, ...	5·5		28		86		83	
Mean temperature of water below intersection, . . .	43°·67	43°·93	42°·74	43°·05	42°·51	43°·08	42°·0	42°·1
Heat in ditto (fathom-degrees), . .	4148	4174	3676	3703	3422	3468	1470	1473·5
Gain of heat in interval, ...	26		27		46		3·5	
Percentage of heat passed downwards, .	473		96		53		4·2	
Heat passed out to air per day, . .	0·30		1·65		3·74		2·77	
Heat passed downwards per day, .	1·44		1·59		2·00		0·12	
Mean temperature of 100 fathoms, . .	44°·33	44°·53	44°·55	44°·54	44°·49	44°·09	44°·09	43°·30

Dates of Maximum Temperature at Different Depths.—The intersections of the curves give, as above shown, an indication of the date of maximum temperature at the particular depth. In Table XII. will be found the depths corresponding to the principal intersections and the mean dates. The actual dates of maximum temperature will always be a day or two later than the mean dates. The number of observations is too small to enable us to say what the maximum temperature at these depths has been.

TABLE XII.

Depth.	Dates of intersecting Curves.		Mean Date.
5	18th August.	5th September.	27th August.
9	18th August.	22nd September.	5th September.
14	5th September.	22nd September.	14th September.
16	5th September.	15th October.	26th September.
19½	22nd September.	15th October.	4th October.
23	22nd September.	14th November.	19th October.
65	14th October.	14th November.	31st October.

As the autumn of 1885 has been a cold one, the above mean dates may be taken as the earliest dates of maximum temperature at the depths indicated.

If we compare the mean temperature of the whole column of 100 fathoms of water at Inversnaid on the 5th and the 22nd September, we have on the 5th the mean temperature 44°·54 F., and on the 22nd September 44°·52, the difference being 0°·02 F. Remembering that heat is lost more quickly in autumn than it is gained in spring, and considering that these temperatures are nearly identical, we shall not be far wrong if we put the epoch of maximum heat in the water of the deepest part of the lake as occurring some time in the third week of September. This brings it very near the date of the equinox, and it seems natural to expect the heat to accumulate in the water so long as the day is longer than the night, and to decrease so soon as the conditions are reversed. This conclusion is supported by the observations of Fischer, Forster, and Brunner, who made a most interesting series of observations on the distribution of temperature in the Lake of Thun, in Switzerland, during the years 1848 and 1849. They found hardly any increase of heat between 3rd February and 28th March, but after the latter date the influx of heat was very rapid. It is probable, therefore, that the temperature of the bottom water in our deepest lakes depends chiefly on the temperature of the air between the preceding autumnal and vernal equinoxes. Two causes combine, namely, the greater meridian altitude of the sun and the greater length of the day in the summer than in the winter half year. The latter cause has the effect that the water is exposed to heating for a greater portion of the twenty-four hours than it is to cooling, while the former cause ensures a greater supply of heat per minute during the day in summer than in winter.

Further, the rate of loss of heat, due to radiation alone, must be very much greater during a winter night than during a summer one.

The rise of temperature in the bottom water and deeper layers is made more apparent by considering the actual readings of the thermometer employed as given by the millimetre scale on the stem.

Depth, . . .		65 fathoms.			85 fathoms.			100 fathoms.		
No. of thermometer,		9			80			21		
Date.	Interval.	Reading.	Difference.		Reading.	Difference.		Reading.	Difference.	
	days	mm.	mm.	F.	mm.	mm.	° F.	mm.	mm.	° F.
September 5, .	...	51·0	...	...	70·1	...	...	60·5	..	...
September 22, .	17	51·6	0·6	0·17	70·4	0·3	0·10	60·4	−0·1	−0·03
October 15, .	23	52·4	0·8	0·23	71·0	0·6	0·21	60·9	+0·5	+0·16
November 14, .	30	52·2	−0·2	−0·06	71·2	0·2	0·07	61·2	0·3	0·09

The position of the index in the thermometers when referred to this scale can be fixed almost to one-tenth of a millimetre, certainly to one-fifth. In thermometer No. 9, 1 millimetre = 0°·285 F.; in No. 80, 1 millimetre = 0°·345 F.; and in No. 21, 1 millimetre = 0°·31 F. The difference of 0·1 millimetre between the readings of No. 21 on 5th and on 22nd September cannot be depended on as real. It is probable that between these dates and during the whole of the summer the temperature had been quite constant. Between 22nd September and 14th November the whole rise of temperature at the bottom is only 0°·25, and it may be confidently affirmed that at a depth of 100 fathoms the whole range of temperature during a single season does not exceed 0°·3 F. By season is meant the summer and winter half of the year, or the period between the date when heating begins in the spring, and that at which the summer heat has been almost wholly lost, and when the water begins again to assume a sensibly uniform temperature from top to bottom. At 85 fathoms the temperature had begun distinctly to rise on 5th September, and by 14th November it had evidently reached about its maximum. At this depth, therefore, the summer range is at least 0°·4 F. At 65 fathoms the water had begun to cool between 16th October and 14th November. In order to determine the range at this depth, it will be necessary to have observations earlier than 5th September.

The bottom temperature has been determined in the deepest part of Loch Lomond by several observers in different years, and the results show that it varies from year to year. James Jardine found it to be 41°·1 F. on the 8th September 1812. Sir Robert Christison found it 42°·0 F. in 1871. I found it to be 40°·2 in April 1872, and 41°·4 F. on 23rd September 1876. About these figures there is always some uncertainty, from the want of comparison between the thermometers. The best evidence of the variation of the temperature of the deep water of our lakes, from year to year, is furnished by my observations in Lochs Lochy (80 fathoms) and Ness (120 fathoms) in five consecutive years in the second week of August. These were all made with the same thermometers, and are to be relied on to one-tenth of a degree. They are—

Years, ...	1877.	1878.	1879.	1880.	1881.
Loch Lochy, . . .	44°·0	43°·7	42°·0	43°·8	42°·25
Loch Ness, . . .	42°·4	42°·3	41°·2	42°·4	41°·45
Mean winter temp. of air at Corran, . .	42°·3	42°·7	38°·9	42°·0	38°·6

The mean winter temperature of the air is the mean temperature of the months of October to March (inclusive) preceding the dates of observation in the lakes. The place of observation is Corran lighthouse, on Loch Linnhe. It is too remote from the lochs themselves to give more than an indication of the climate at the surface of the lakes. Still the bottom temperature of Loch Ness does follow very closely the mean winter temperature at Corran. In the two severe winters, 1878–79 and 1880–81, the mean winter temperatures fell below 39°, and as might have been expected the temperature of the deep water of the lake was slow to follow it, owing to the change in the properties of water at this temperature. Hence the higher the mean temperature of the cold months of the year is, the more closely is it reproduced in the deep water of the lake. Forty years ago Amié showed that the temperature which he observed in the abyssmal regions of the Western Mediterranean agreed sensibly with the mean winter temperature of the air at its surface. The later view, which ascribed the temperature of the deep water in the Mediteranean to the Atlantic water flowing over the ridge at Tarifa, which has the same temperature as the Mediterranean water within the ridge, left out of account the fact that there is on the whole an outflow of pure Mediterranean water at the bottom which affects the temperature and density of the Atlantic water outside. In sea water, owing to its saltness, convection currents are set up more actively by cooling than is found to be the case in fresh water. In lakes, however, and especially in those situated in mountainous districts, the production of convection currents is powerfully assisted by local differences of climate. These are due to differences of exposure, to radiation, and to prevailing winds. Such local differences of climate produce local differences of temperature, and consequently of density in the superficial layers. If we compare, for instance, the observations made at Ardlui and at Inversnaid on 15th October, we find that the mean temperature of the first 20 fathoms is 48°·24 at Inversnaid, and only 46°·58 F. at Ardlui. The stations are only three miles apart, and yet there is a difference of 1°·66 F. in the mean temperature of the first 20 fathoms. Another cause affects the distribution of temperature in a lake, namely, the drainage, but this more particularly affects shallow lakes or basins. It is probable that the most powerful means of supply and removal of heat is direct radiation. The most powerful mixing agency is the wind.

I hope, with Mr Morrison's assistance, to secure monthly observations during the coming winter and spring, the results of which cannot fail to be interesting.

2. On Oceanic Shoals discovered in the s.s. "Dacia" in October 1883. By J. Y. Buchanan, F.R.S.E.* (Plate XII.)

Owing to the kindness of the India Rubber, Gutta Percha, and Telegraph Works Company of Silvertown, and especially of their engineer-in-chief, Mr Robert Kaye Gray, I was enabled to accompany their expedition for laying the telegraph cable between Cadiz and the Canary Islands. It consisted of two steamers belonging to the company, the "Dacia" and the "International"; and I was allotted a place and all facilities for working on board the "Dacia," where Mr Gray had charge. I joined the expedition at Cadiz on 3rd October 1883.

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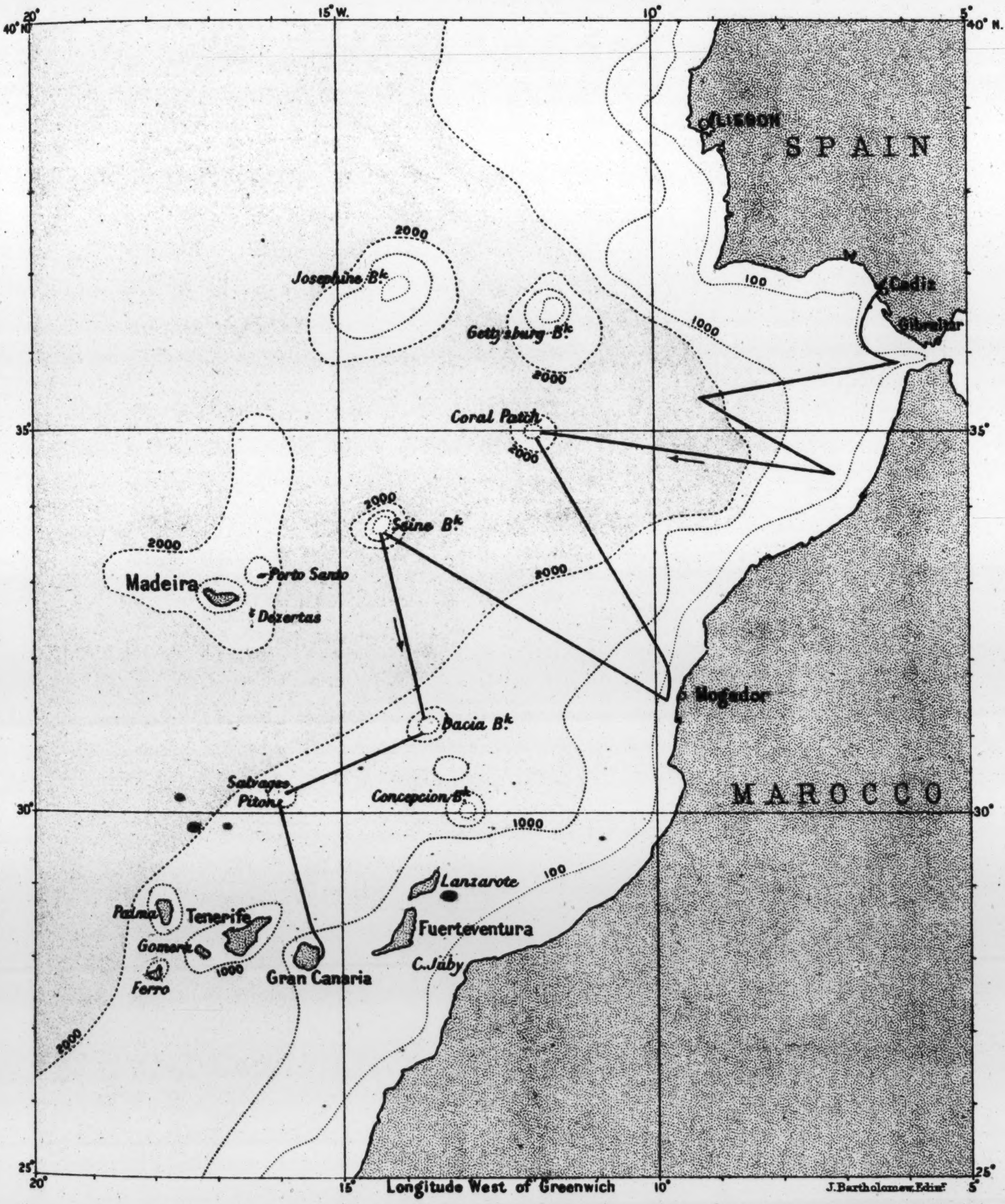
* A portion of the narrative part of this paper appeared as a correspondence in the *Times* of 7th December 1883.

Proc. Roy. Soc. Edinr. Vol. XIII. Plate XII.



The contour lines indicate the depths in fathoms

———— Track of the "Dacia" October, 1883.

The "Dacia" left Cadiz on the afternoon of the 4th October. The entrance to the Straits of Gibraltar was reached the next morning, and two days were spent in thoroughly sounding its shallowest part, which lies nearly on a straight line connecting Cape Spartel on the African with Cape Trafalgar on the European coast. On existing published charts the bottom is seen to be very uneven. From Cape Trafalgar south, for more than half the distance across, the water does not exceed 100 fathoms in depth; then it deepens to 150 fathoms, suddenly shoals to 45 fathoms, then deepens again, and remains deep till close to the African shore. In order to assist in fixing the positions of the soundings, a buoy was anchored on the mid-channel bank. It remained down for two days. When it was brought on board again, the thick wire mooring-rope was found nearly chafed through by the violent currents rubbing it on the hard coral bottom. During the two days spent on this ground, a large number of soundings were taken, the result of which was to define and enlarge the mid-channel shoal from a patch a mile and a half long and half a mile broad to a bank 7 miles long from east to west by 2 miles broad from north to south; further to show that the deep water runs between this bank and the African shore, and that the greatest depth on this, the shallowest ridge, is not less than 200 nor more than 210 fathoms. Considerable interest attaches to a knowledge of this depth. It has been supposed that on it depends the temperature of the deep water of the Mediterranean, as on it would depend the temperature of the coldest water which could find entrance into it from the Atlantic. In the Atlantic, however, outside the Straits, the temperature of the water at 200 fathoms below the surface is decidedly lower than that of the deep water of the Mediterranean. Moreover, it was proved by the observations of Sir George Nares and Dr Carpenter, in Her Majesty's ship "Shearwater," that, though the currents in the Straits are affected by the tides, there is, on the whole, an inflow of Atlantic water at the surface and an outflow of dense Mediterranean water at the bottom. It was also shown by M. Aimé, in 1848, that the temperature of the deep water of this part of the Mediterranean agrees with the mean temperature of the coldest half of the year at its surface. It is probable, therefore, that the deep overflow of the Mediterranean has more effect in raising the temperature and density of the deep water of the Atlantic than the Atlantic has in reducing that of the Mediterranean. The observations made on board the "Dacia" quite bear out this view.

The observations of bottom temperature made by the "Challenger" in the north-eastern part of the Atlantic, and by the "Dacia" in this restricted part, show a higher temperature and density of the water at great depths than is found to exist in any other part of the ocean. Although other causes are at work to produce this, it cannot be doubted that the warm and dense overflow of the Mediterranean has its share in its production, especially in its immediate neighbourhood.

From the Straits of Gibraltar a westerly course was steered for about 150 miles, then a south-easterly one until the African coast was sighted, when the course was again altered to a westerly one. It was the intention to have taken soundings and other observations along this line for a distance of 180 miles. On continuing the line representing the course on the chart, it was found to cut the line of soundings taken by the steamship "Seine" between Lisbon and Madeira close to a point where the bottom seemed to rise from a depth of 2400 fathoms to 1800 fathoms.

This line of soundings was run previous to the laying of the cable from Lisbon to Madeira by the s.s. "Seine." The soundings were taken at distances of about 25 miles apart, and showed a tolerably level bottom with an average depth of rather over 2000 fathoms. Slight unevennesses had been observed, such as two successive soundings differing by 300 fathoms, but it never occurred to any one to suppose that they indicated anything that could interfere with the laying of the cable or its security when laid; and accordingly the cable was laid over the line sounded. During the process, and when the ship was passing over one of the unevennesses indicated, where the depth seemed to fall from 2400 fathoms to 1967 fathoms, and to increase again to 2332 fathoms, the cable suddenly parted; and, on making a sounding, a depth of *one hundred* fathoms was found in place of 2000 expected. The sudden shoaling of the water had naturally snapped the cable. No blame could be attached to those in charge of the work, because the soundings, on which they based their calculations, were considered sufficiently close, and were supposed to give quite sufficient guarantee of a suitable bottom.

While the "Dacia" was running the second long line to seaward it was found that, on prolonging it, it cut this Lisbon-Madeira line of soundings at a point where there was a shoaling from 2400 to 1800 fathoms.

Before the discovery of the "Seine bank," with under 100 fathoms of water on it, there was no indication of its existence except a similar shoaling from 2322 to 1967 fathoms. It was thought, therefore, that another similar bank might exist, and the ship was kept on the same course with the view of going as far as the "Seine's" 1885 fathoms sounding. At the point where it had been intended to turn, a depth of 2400 fathoms was found; at the next sounding, 50 miles farther west, bottom was struck in 485 fathoms. This depth was found at 4 A.M. on the 12th of October, and the sounding was immediately repeated, with the same result. The whole day was then devoted to the exploration of the bank thus revealed.

The discovery of this bank or "Coral patch" may fairly be claimed as a success in marine diagnosis. The shoalest water found on it was 435 fathoms, in lat. 34° 57′ N., long. 11° 57′ W., and the depth ranged up to 600 fathoms. The shallow water extends for a distance of 6 miles in an east and west direction, and about 3½ miles in one from north to south. On the western edge it seemed to fall away precipitously from 550 to about 850 fathoms, when the slope became gentle, and the bottom changed from hard coral to soft ooze. In one sounding on this ledge the sinker distinctly struck bottom in 550 fathoms, tumbled over and continued to sink, struck in 620 fathoms, again tumbled over, and finally found a resting place in 835 fathoms. When it came up it had a large brownish-black streak, where it had evidently struck obliquely on manganese rock. This was a very remarkable sounding, and quite undoubted. All the conditions were the most favourable—no wind, the sea calm, and the ship motionless. A grapnel with extemporised dredge and hempen swabs was put over in 530 fathoms, and on being brought up the dredge was found much torn, and on the swabs a large quantity of beautiful white coral and many fragments of a crinoid. The coral was examined by Professor Moseley, and determined as *Lophohelia prolifera*. It was found to be growing luxuriantly, the living stalks being rooted on dead and decaying branches of the same species. The dead branches were in many instances beginning to get coloured brown from deposited manganese. One of the earliest and most interesting of the "Challenger's" dredgings was in 1500 fathoms, about 200 miles south-west of Teneriffe. The dredge came up full of beautifully branching jet-black coral attached to a soft black rock. Thickly clustered among the branches were large siliceous sponges, like masses of spun glass. Both the rock and the enamel-like coating of the coral consisted of black oxide of manganese. The same has been found on the ridge connecting Teneriffe and Grand Canary at a depth of 1000 fathoms, and after severe storms it is sometimes washed ashore on the islands.

It is not unlikely that if the ground about the "Challenger" station in 1500 fathoms were closely and carefully explored a very interesting shoal might be discovered.

The fragments of the crinoid obtained were determined by Dr Herbert Carpenter to be *Actinometra pulchella*, which is common in the Caribbean Sea. One specimen of it had been previously got by the "Porcupine," in 477 fathoms, near Gibraltar.

The temperature of the water was taken at different depths with the following results:—

Depth (fathoms),	Surf.	50	100	150	300	500
Temperature (Fahr.),	68°·5	60°·2	56°·8	54°·8	51°·9	50°·0

After exploring this coral bank, so far as time permitted, the ship was directed towards Mogador, on the Morocco coast. Independently of the high land, which is visible for many miles at sea, the approach to the coast is indicated by a rapid fall in the temperature of the water of the sea surface and a remarkable change in its colour. Outside the temperature of the surface was very constantly 69° F. After sighting the land it fell at first slowly, then rapidly, and when two miles from Mogador it was only 61° F. The presence of this body of cold water must have an important effect in moderating the climate of places situated on this somewhat desert coast. In the open sea, and away from shore influences, the ocean water in all moderately warm latitudes has the same deep, transparent, ultramarine colour. In colder latitudes this becomes greenish-blue and green, and the water loses its transparency. The colour of the water off the Morocco coast is particularly remarkable, for it becomes of a deep and, at the same time, very transparent olive-green colour, unusual in any but icy latitudes. Closer in shore it loses its transparency.

A similar phenomenon is observed on the west coast of South America. Close in shore all the way from Valparaiso to Cape Blanco, in lat. 4° 30′ S., there is a fringe of cold, green, comparatively fresh water teeming with life. In the course of a voyage along that coast in April 1885 I found the temperature of the water identical in the harbours of Coquimbo in lat. 30° S., and Payta in lat. 5° S., namely, 63°·5 F. This is an exceedingly low temperature even for the more southerly of the two stations, and is of course very much more remarkable for the more northerly one, Payta, which indeed may be said to be almost on the equator. Along this coast to the southward of Callao there is no marked current or stream setting in either direction. In order to bring water of 63°·5 F. by a surface current to so low a latitude as 5° S. would require a rapidity of transport which, under the circumstances, is inconceivable. Similarly on the African coast the set of the water along the shores of Morocco is not towards the equator, but towards the north; and it was observed by Sir George Nares, in H.M.S. "Shearwater," that this cold, in-shore water actually penetrates past Cape Spartel into the Mediterranean.

In low latitudes there is only one source of cold water, namely, the climate of higher latitudes. The water cooled in these high latitudes may make its way towards the equator either as a surface current or as a movement of the deeper waters. If it is conveyed as a surface current, it is exposed to the heating on the way in passing through regions of progressively warmer climates, and in order to keep a low temperature into anything like low latitudes it would require to form a current of great volume and velocity. In all latitudes we find almost ice-cold water at a few hundred fathoms from the surface, and in fact the nearer we come to the equator the nearer does the cold water come to the surface. In the latitude of Payta, and away from the coast, water of 60° F. would naturally be found at a depth of 100 fathoms; at the surface and in the open ocean it would not be found at a less distance than 2000 miles.

The western coasts of the continents are the *weather shores* of the oceans. Both the continents mentioned occupy positions which extend from the northern trade-wind region through the equatorial calms to the southern trade-wind region. The trade winds blow from the north-east and the south-east, and the result of their mechanical action is to drag away the water from their weather shores and drive it to leeward. As the wind acts at the surface it removes chiefly surface water, and it is the surface water which assumes the temperature due to the local climate. The water so removed must be supplied, and it is supplied from the readiest source. As the surface water is being removed by the wind, it draws on the deeper layers to supply the deficiency. But in low latitudes the deeper water in the ocean has always a very much lower temperature than that at the surface. Hence, at the weather shores of the oceans we have a constant removal of surface water to leeward and replacement of it by colder water from greater depth. The cold water found along the west coast of South America has always been considered evidence of the "Humboldt current." And so it is. But the Humboldt current, or at least the current which brings the cold water there, is not a horizontal current from higher latitudes, but a vertical one from greater depths.

From Mogador a course was shaped for the Seine bank above referred to. It is indicated on the chart by two soundings of 100 and 118 fathoms respectively, at a distance of 12 miles apart. The bank was struck on the position of the 118 fathom sounding, and a day was devoted to its exploration. A couple of "balloon buoys" were anchored in the noon position, namely, lat. 33° 47′ N., long. 14° 1′ W., the depth being 89 fathoms. Hempen swabs were attached to the end of the anchoring line. These balloon buoys are made of india-rubber and canvas, and when not in use occupy very little space. They are used for floating the shore end of a cable from the ship to the land, and they are then inflated with air by means of a small pump. They are much like footballs 3 feet in diameter. The bank was crossed and recrossed in different directions, and found to occupy less surface than was expected. Its length in an approximately north and south direction is 6 miles, and its breadth from east to west 3 miles. The depth of water over an area of about 15 square miles varies only from 86 to 100 fathoms. When the sounding work was done the buoys, with their moorings, were got on board, and the swabs, which had been brought up without dragging, were found to contain a rich harvest of large crinoids of a delicate, reddish colour. Dr Herbert Carpenter has determined them to be *Antedon phalangium* of the Mediterranean and the Atlantic coasts of Spain. It was obtained by the "Porcupine" as far north as the Minch and the Faerœ Banks. Along with the crinoids pieces of broken coral and shells came up, all stained of an intense yellowish-green colour, which was freely communicated to the spirit into which the shells were put for preservation. The colouring matter so extracted was examined by Professor Hartley of Dublin, and found to be chlorophyll. Specimens of spirit coloured by deep-sea animals collected on the "Challenger" expedition were also examined, in consequence of this discovery, and they also were found to have the spectroscopic character of chlorophyll.

The bottom temperature on the Seine bank was 60°·2 F. On leaving the Seine bank, the intention was to have reached in towards the African coast, then outwards to the small group of Salvage and Piton, and then to make a straight course to Grand Canary. The day after leaving the Seine bank the value of marine diagnosis was again vindicated. When about 170 miles south of the bank, a sounding gave 1189 fathoms with hard bottom, where at least 1800 fathoms were looked for. Another bank was immediately suspected. Three miles farther, on the same course, 1386 fathoms were found. If a bank existed it had, therefore, been passed over. The course was immediately reversed, and after steaming 7 miles back a sounding gave 810 fathoms; 3 miles farther back 414 fathoms were found, and 2 miles farther 66 fathoms. Half a mile beyond this sounding 230 fathoms were found. The ship was again turned round and steered to the southward for about a mile and a half, when a buoy with lights was put over in 175 fathoms, and, as it was already past midnight, the ship lay by it till daylight. As this bank lay very close to the proposed line of cable, two days were devoted to its exploration. It was found to be of an irregularly triangular shape, broader towards the north and tapering towards the south. Its greatest length from north to south and breadth from east to west were found to be equal, and 8½ miles respectively, while the total area, with less than 100 fathoms of water on it, was 50 square miles. A boat was anchored in the noon position of the 21st October—namely, lat. 31° 9′ 30″ N., long. 13° 34′ 30″—in 58 fathoms of water. This is near the centre of the bank, and a little to the southward of the shoalest sounding, 49 fathoms; swabs had been attached to the mooring rope of the buoy; they had also been attached to the moorings of a couple of balloon buoys in rather shallower water, but during the two days that they were down the ropes got so chafed that they broke when being recovered. Swabs had been attached to the boat's cable, and they were recovered, but, as they had been only a very short time on the bottom, little was expected and little was found on them. A few broken and decayed shells made up the harvest.

Advantage was taken of the buoy being anchored in mid-ocean to make some current observations. It is an almost universal experience of ships making a passage from the north to the Canary Islands to be set very considerably to the eastward of their course, and many disastrous shipwrecks have been the consequence. It appears from recently-collected observations that the general set of the current is about E.S.E., and its velocity averages 16 miles per day. Only a few weeks previously a buoy belonging to the United States Lighthouse Department had been washed ashore on the north coast of Teneriffe. Knowing how the tidal wave is transformed into a current on approaching the coast, it seemed not unlikely that the occurrence of so extensive a shoal as the "Dacia bank," as it has been called according to recognised precedent, might produce the same effect. Accordingly, before the buoy was brought on board again, I spent some hours in a boat riding to it, and determined the direction and rate of the surface current at frequent intervals, while the ship went on with the sounding at a distance. The result of these observations was, shortly, to show that the south-easterly current in this part of the ocean is affected and at times reversed by a tidal current. Down to a depth of 70 fathoms the current was found to be setting in nearly the same direction as at the surface, but with somewhat greater strength. The "current drag" used on this occasion was a "tow-net" made of fine but stout muslin, about 3 feet long and 1 foot diameter at the mouth, tapering to a cone in the bag. The mouth was kept open by a stout iron ring, which was made fast to the sounding line which passed diametrically across it. The water was of so clear and transparent a blue that it could be distinctly seen down to a depth of 19 fathoms; and, with a weight of 14 lb. at the end of the line, it set itself perfectly steadily and almost rigidly in its direction.

The results of the observations of the current at the surface on 21st October 1883 were—

Hour P.M., . . .	2.15	2.40	3.30	4.6
Direction (true), . .	N. 11° E.	N. 41° E.	N. 56° E.	N. 101° E.
Rate, knots per hour,	0·47	0·30	0·26	0·30

It will be seen from these observations that, in the two hours, the current had shifted its direction through 90°, and had passed through a minimum velocity of 0′·26 per hour without there having been any period of "slack water." The observations are too few in number to make it worth while submitting them to analysis, but a little study of them will show that they indicate a current which is the resultant of a constant current and a periodic one. A constant current running to S.E. by E., combined with a tidal current running N.N.W. and S.S.E., the maximum velocity of which, in either direction, is twice that of the permanent current, would give a resultant current agreeing fairly with that observed. Under these circumstances, the path of a particle, floating freely in the water and moving with the water, would describe in twenty-four hours a path having the shape of the letter S, the larger axis of which has a N.W. by W. and S.E. by E. direction. If the tidal current be supposed to have motion in an elliptical orbit, instead of a simple oscillation in one direction, the path in the twenty-four hours would take some such form as the figure 3. Without insisting further on the numerical value of the results, the observations are of importance as showing decidedly the existence of tidal currents in the open ocean. Amongst the islands of the Canary group these currents are strongly developed. Between Grand Canary and Teneriffe the channel is broad, and, at its shallowest part, over 1000 fathoms deep; yet the tidal current reaches to the very bottom, and its scouring action is shown by the nature of the bottom. To seaward, in 1800 or 2000 fathoms, the bottom is a fine Globigerina ooze, which gets coarser and sandier as the water shoals in the channel, until, on the summit of the ridge, there is generally no deposit at all, and the bottom is rock or coral often coated with black oxide of manganese.

An essential condition of the existence of coral, whether in the deep sea or in the shallow water of the atolls of tropical seas, seems to be circulation of the water. This serves the double purpose of bringing food and removing sediment. In shallow water this circulation is produced by the breaking of ordinary storm waves on the shore or on shoals; in deep water it is produced by the transformation of the tidal wave into a current on meeting with obstructions such as ridges or banks, even though they may have a thousand or more fathoms of water on them. In both cases the current is produced in the same way, namely, by the annihilation of a wave.

In Mr Murray's theory of the formation of coral islands, the growth (in height) of a shoal, owing to the greater amount of sediment which reaches it from the surface compared with the amount which can reach an equal area of ground at a greater depth, is an important feature. We have seen, however, that it is probable that, wherever the bottom rises in the path of the tidal wave, a portion of the wave is transformed into current. This current would naturally keep the summit of the shoal clean swept of all freshly falling sediment, and would thus tend to limit the growth of such a shoal.

But this very agency must favour the existence of such animals as deep-sea corals on the areas kept clear of sediment. It seems reasonable, therefore, to expect that such areas would be occupied by deep-sea corals. These would not be likely to extend laterally beyond the cleared summit of the rising, but would grow upwards, the living resting on the débris of the dead. The tendency would be to raise a massive pillar with perpendicular sides up through the water, and it would grow until the conditions, principally of temperature, set a limit. In the "Coral patch" we have undoubtedly such a structure, the base of which rests on the crest of a hill 800 fathoms below the surface. Its summit is now rather over 400 fathoms below the surface, and it is in full growth. Judging from what could be obtained from the surface of the Seine bank and the Dacia bank, they have reached limiting conditions. Had they risen to their present level in equatorial regions, they would doubtless have been occupied by reef-building species, and would have formed true coral islands.

We have seen above that, on the Coral patch, the lead was dropped by chance on the face of a precipitous cliff at least 200 fathoms high. On the Dacia bank the mark buoy happened to be let go just on the edge of the bank in 175 fathoms. On trying to lift the moorings the buoy-rope carried away, and it was found to have been chafed through about 100 fathoms from the surface, or 75 fathoms above the ground. The currents had evidently been rubbing it against the edge of the cliff during the two days that it was down. From the mark buoy the "Dacia" steamed west for about 10 miles, taking up and down soundings every mile, and "flying soundings" with a pneumatic sounding-machine which I constructed on board. It worked extremely well with the ship going 7 knots, and in water ranging from 50 to 100 fathoms in depth. About 8 miles west of the buoy, the edge of the bank was again struck. After an up and down sounding of 86 fathoms, a flying sounding gave 110 fathoms. The engines were immediately stopped and put astern, when an up and down sounding gave 333 fathoms. The distance between these soundings could not have been more than a quarter of a mile, which gives an average gradient between the soundings of nearly 45°. To the westward of the 333 fathoms sounding, 619 fathoms were found at 1 mile, and 844 fathoms at 2 miles. At this sounding a 1½-inch mud tube was used, and it brought up a good sample of very coarse-grained Globigerina ooze, with many pteropod and other shells.

In the following table will be found the slopes on the Dacia and the Seine banks, as observed between adjacent soundings, also those on the slope of Bermuda and adjacent banks, taken from the most recent Admiralty charts:—

Seaward Slopes of Oceanic Islands and Shoals.

Locality.	Slope.	Depth Intervals.		Differ-ence.	Distance between Soundings.	Tangent of Angle of Slope.	Angle of Slope.
		From Fms.	To Fms.				
Dacia bank.	West.	110	333	223	0°·25	0·92	43°
,,	,,	330	619	289	1·00	0·29	16
,,	,,	619	884	225	1·00	0·23	13
,,	S.W.	97	629	532	1·50	0·36	20
,,	South.	79	335	256	0·50	0·51	27
,,	,,	335	757	422	1·50	0·28	16
,,	East.	100	200		Precipitous.		
,,	,,	200	430	230	0·25	0·92	43
,,	,,	430	605	175	1·50	0·12	7
,,	,,	605	1102	479	2·00	0·25	14
,,	North.	72	306	234	0·75	0·31	18
,,	,,	306	751	445	2·00	0·22	13
Seine bank.	South.	107	530	423	1·00	0·42	23
,,	East.	120	598	478	2·00	0·24	13
,,	N.E.	99	215	116	2·00	0·06	3
,,	,,	215	679	464	2·00	0·23	13
,,	North.	111	1149	1038	3·00	0·35	19
Bermuda.	S.E.	115	420	305	0·45	0·68	34
,,	,,	27	315	288	0·45	0·64	33
,,	,,	55	307	252	0·60	0·42	23
,,	,,	26	329	303	0·70	0·43	24
,,	,,	326	400	71	0·45	0·10	5
,,	,,	33	160	127	0·20	0·64	33
,,	,,	160	380	220	0·60	0·37	21
,,	,,	35	355	320	0·65	0·50	27
,,	,,	355	780	425	1·60	0·27	15
,,	,,	24	240	216	0·60	0·36	20
,,	,,	18	200	182	0·55	0·33	19
,,	,,	20	380	180	0·60	0·30	17
,,	,,	320	435	115	0·70	0·17	9
,,	,,	60	235	175	0·45	0·39	22
,,	,,	11	260	251	0·28	0·89	42
,,	N.E.	30	225	195	0·30	0·65	33
,,	,,	16	180	164	0·40	0·41	23
,,	,,	180	250	70	0·20	0·35	19
,,	,,	21	137	116	0·25	0·46	25
,,	,,	25	145	120	0·25	0·48	26
,,	West.	42	1950	1908	3·50	0·55	29
,,	South.	26	230	204	0·40	0·51	27
,,	,,	506	1075	569	2·30	0·25	14
,,	,,	30	950	920	2·70	0·29	17
Challenger bank.	West.	36	1350	1314	2·50	0·52	28
,,	,,	1350	2175	825	3·70	0·22	13
,,	North.	175	1250	1075	1·30	0·83	40

Bermuda is a coral island, crowning an eminence, which rises abruptly out of the deepest water of the North Atlantic. Besides the island of Bermuda, this eminence carries two shoals or banks to the south-west of the island. These banks seem to exactly resemble the "Dacia" and "Seine" banks. The minimum depth on the "Challenger" bank is 24 fathoms, and on the "Argus" bank, which lies farthest to the southward, is 10 fathoms. The separating channels have a depth of 1000 and 600 fathoms respectively. On the reef, which surrounds the island of Bermuda, the water deepens very slowly to 25 fathoms. As soon as this depth is passed, the declivity becomes very steep, and is no doubt in many places precipitous. The mean angle of slope for depth-intervals between 34 and 344 fathoms is 25°·3, between 29 and 239 fathoms it is 26°·4, and between 24 and 155 fathoms it is 27°. The western and northern sides have a steeper average slope than the southern and eastern ones. On the western side we have, from 42 fathoms on the reef to 1950 fathoms, the enormous average slope of 29°; and on the north side of the "Challenger" bank, between the depths 175 and 1250 fathoms, we have an angle of 40°. It is difficult to believe that these average slopes are not made up of large portions which are really vertical, with gentler slopes at greater depths. The closer soundings on the "Dacia" bank and Coral patch revealed such precipices.

While the "Challenger" was at Tahiti, very careful soundings were made to determine the actual slope of the reef. At a distance of 125 fathoms from the edge of the reef, a depth of 30 to 35 fathoms is found; at 150 fathoms from the reef the depth is from 90 to 100 fathoms. In this interval the slope attains its maximum angle of from 60° to 70°. "It is believed to have been formed by huge masses and heads of coral which have been torn away from the ledge between the edge of the reef and 35 fathoms during storms, or by overhanging masses which have fallen by their own weight. In this way a talus has been formed on which the corals, living down to 35 fathoms, have found a foundation on which to build farther seawards, for this seaward slope is the great growing surface of the reef."* If the steepest part of the slope is a talus, it is evident that the solid rock face inside the talus must be still steeper, and a slope steeper than 70° is a precipice.

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* *Challenger Narrative*, vol. i. p. 781.

Seaward Slopes on the Morocco Coast.

Lat. N.	Depth inward.		Difference.	Distance.	Tangent of Angle of Slope.	Angle of Slope.
	From Fms.	To Fms.				
33° 20′	526	1964	1438	8′·0	0·180	10°
	1964	2338	374	8·0	0·047	2¾
33 10	224	636	412	4·0	0·103	6
	636	386	−250	2·0	−0·130	−7½
	386	768	382	5·0	0·076	4½
	768	1095	327	4·0	0·082	4¾
	1095	2060	965	10·0	0·097	5½
32 40	664	787	123	3·0	0·041	2½
	787	1050	263	2·0	0·131	7½
	1050	1325	275	6·0	0·046	2¾
32 15	220	600	380	2·0	0·190	11
	600	892	292	7·0	0-041	2½
	892	1290	398	5·0	0·080	4¾
	1290	1405	115	5·0	0·023	1¼
	1405	1620	215	8·0	0·027	1½
32 0	106	220	114	2·0	0·057	3¼
	220	596	376	9·0	0·041	2½
	596	780	184	5·0	0·036	2
	780	843	163	4·0	0·041	2½
	843	995	138	3·0	0·046	2¾
	995	1640	645	14·0	0·046	2¾
	1640	1763	123	10·0	0·012	⅔

The soundings off the west coast of Morocco which are given in the second table were taken too far apart to give anything more than rough averages of the slope. They indicate, however, a feature of continental slopes which calls for further and minute investigation. Roughly speaking, the sea bottom slopes very gently from the coast-line to a depth of 100 fathoms; between 100 and 500 fathoms a steep descent is met with. From 500 to 700 or 800 fathoms the angle of slope is smaller, and between 800 and 1200 fathoms it again becomes steeper, and flattens between 1200 and 1500 fathoms, sometimes becoming again steeper below 1500 fathoms. From the continental profiles which we have, the existence of these *terraces* seems to be very probable. It is to be hoped that, in future work, the soundings will be made so close to one another that this matter may be put out of doubt.

In the accompanying map (Plate XII.) the positions of the banks found and explored by the "Dacia" are given, as well as the "Josephine" and the "Gettysburg" banks, lying nearly in the latitude of the Straits of Gibraltar, the former with 80 and the latter with 30 fathoms. In the *North Atlantic Directory** a chapter is devoted to cataloguing the shoals and rocks which have been reported to have been found by different navigators in the open ocean. The great majority of these are very doubtful, but there are one or two which it would be exceedingly interesting to explore, as their existence rests on positive evidence. I would particularly mention the Chaucer bank, in lat. 42° 45′ N., long. 29° W. It was discovered by Captain Robert Henderson of the ship "Chaucer," on a voyage from Mauritius to Glasgow, on 28th October 1850. Having noticed he water discoloured, he sounded and found hard bottom at 48 fathoms. Two hours afterwards he sounded again in 50 fathoms, and two hours later he found bottom in 70 fathoms. These soundings put the existence of the "Chaucer" bank beyond a doubt. Similarly the Sainthill bank, in lat. 42° 37′ N., long. 41° 45′ W., on which Captain Sainthill, R.N., got a good sounding in 100 fathoms, hard bottom; and the Milne bank, in lat. 43° 35′ N., long. 38° 50′ W., on which Admiral Milne got three soundings in 92, 81, and 100 fathoms, with fine sand and ooze, obviously exist, though they may perhaps form parts of one and the same shoal. The soundings of the "Dacia" show that obtaining one or two deep soundings in the neighbourhood affords no evidence whatever of the non-existence of a reported or suspected shoal. When a ship is sent to look for such a shoal she must be fitted with the necessary apparatus for taking deep soundings with ease and rapidity. Arrived on the ground, she must start sounding at close intervals, and always follow the lead of her own soundings, proceeding in the direction in which they get shallower until she either finds the shoal or the water deepens again. This method was used with great success, and by it the Coral patch, and more particularly the "Dacia" bank, were discovered.

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* Findlay's *Atlantic Directory* (1865), p. 558.

3. On the Phylogeny of the Tunicata. By W. A. Herdman, D.Sc., F.L.S., Professor of Natural History in University College, Liverpool.

(*Abstract.*)

This paper deals with the relationships between the different groups of Tunicata, and the attempt is made to trace their phylogeny, and to construct a tree-like figure, showing the course of evolution of the group, and based upon anatomical and enbryological observations. The following are the chief results which are discussed :—

The Proto-Tunicata were derived from the Proto-Chordata by degeneration and modification, and they are represented at the present time by the Appendiculariidæ.

The Proto-Thaliacea and the Proto-Ascidiacea diverged in two different directions from the Proto-Tunicata, close to the ancestral Appendiculariidæ. The Doliolidæ and the Salpidæ form two divergent lines from the Proto-Thaliacea. *Anchinia* is an offshoot from the ancestral Doliolidæ.

The Proto-Ascidiacea gave up their free-swimming pelagic mode of life and became fixed. This ancestral process is repeated at the present day by the free-swimming larva of the fixed Simple and Compound Ascidians. The Proto-Ascidiacea are probably most nearly represented at the present day by the genus *Clavelina.* They have given rise, directly or indirectly, to the various groups of Simple and Compound Ascidians.

Two chief divergent lines arose from the ancestral Clavelinidæ—one leading to the more typical Compound Ascidians, and the other to the Ascidiidæ and other Simple Ascidians. The first of these ancestral lines gave rise to *Diazona* and the Distomidæ, and later on to the primitive Didemnidæ. The Polyclinidæ form a side branch from the base of the primitive Distomidæ. One important new family, the Cœlocormidæ (formed for the reception of *Cœlocormus huxleyi*, obtained off the east coast of South America, from a depth of 600 fathoms, during the "Challenger" Expedition), was derived from the primitive Didemnidæ, and in its turn gave rise to the Pyrosomidæ, thus connecting the aberrant and highly modified *Pyrosoma* with the Distomidæ, the most typical Com-

pound Ascidians. The Diplosomidæ were derived from the ancestral Didemnidæ.

The remaining two families of the Compound Ascidians, the Botryllidæ and the Polystyelidæ, were derived independently from the ancestral Simple Ascidians, and are therefore not closely allied to the Polyclinidæ and the Distomidæ. Probably the Botryllidæ arose from the primitive Cynthiidæ which were derived from the Ascidiidæ. The Polystyelidæ, on the other hand, form a modified offshoot from the Styelinæ. The new "Challenger" genus *Chorizocormus* shows how the transition may have been effected from such a Simple Ascidian as *Polycarpa* to such a Compound Ascidian as *Goodsiria.*

The Proto-Ascidiacea were probably colonial forms, and gemmation was retained by the Clavelinidæ and by the typical Compound Ascidians (Distomidæ, &c.) derived from them. The power of forming colonies by budding was lost, however, by the primitive Ascidiidæ (Simple Ascidians), and must, therefore, have been regained independently by the ancestral forms of the Botryllidæ and the Polystyelidæ derived from the Simple Ascidians. In the Polystyelidæ the differentiation of the colony has not gone so far as in most other groups of the Compound Ascidians, and the Ascidiozooids have not become arranged in true systems.

The ancestral Cynthiidæ split into two lines of descent—one leading to the Styelinæ and the Polystyelidæ, and the other to the primitive Cynthinæ. This latter group is represented most nearly at the present day by the genus *Cynthia,* while the Bolteninæ form a side branch. The Molgulidæ were also derived from the primitive Cynthinæ; they have undergone considerable modification.

In the paper the details of the changes which the various families and genera have probably undergone during their evolution are discussed, and the more important hypothetical ancestral forms are considered. The two most striking conclusions which have been arrived at are—(1) the relationship of *Pyrosoma* with the Distomidæ through *Cœlocormus;* and (2) that the Compound Ascidians are a polyphyletic group derived from the ancestral Simple Ascidians at three distinct points.

4. On Dew. By Mr John Aitken.

(*Abstract.*)

The first point referred to in this paper is the source of the vapour that condenses to form dew. A few observations of the temperature of the ground near the surface and of the air over it, first raised doubts in the mind of the author as to the correctness of the now generally-received opinion that dew is formed of vapour existing at the time in the air. These observations, made at night, showed the ground at a short distance below the surface to be always hotter than the air over it; and it was thought that so long as this excess is sufficient to keep the temperature of the surface of the ground above the dew-point of the air, it will, if moist, give off vapour; and it will be this rising vapour that will condense on the grass and form dew, and not the vapour that was previously present in the air.

The first question to be determined was whether vapour does, or does not, rise from the ground on dewy nights. One method tried of testing this point was by placing over the grass, in an inverted position, shallow trays made of thin metal and painted. These trays were put over the ground to be tested, after sunset, and examined at night, and also next morning. It was expected that, if vapour was rising from the ground during dewy nights, it would be trapped inside the trays. The result in all the experiments was that the inside was dewed every night, and the grass inside was wetter than that outside. On some nights there was no dew on the outside of the trays, and on all nights the inside deposit was heavier than the outside one.

Another method of testing this point was employed, which consisted in weighing a small area of the exposed surface of the ground, as it was evident that if the soil gave off vapour during a dewy night it must lose weight. A small turf about 6 inches (152 mm.) square was cut out of the lawn and placed in a small shallow pan of about the same size. The pan with its turf, after being carefully weighed, was put out on the lawn in the place where the turf had been cut. It was exposed for some hours while dew was forming, and on these occasions it was always found to lose weight. It was thus evident that vapour was rising from the ground while dew was forming, and therefore the dew found on the grass was formed of part of the rising vapour, trapped or held back by coming into contact with the cold blades of grass.

Another method employed for determining whether the conditions found in nature were favourable for dew rising from the ground on dewy nights, was by observations of the temperatures indicated by two thermometers, one placed on the surface of the grass, and the other under the surface, amongst the stems, but on the top of the soil. The difference in the readings of these two thermometers on dewy nights was found to be very considerable. Ten degrees was frequently observed, and occasionally it was as much as eighteen degrees. A minimum thermometer placed *on*, and another *under*, the grass, showed that during the whole night a considerable difference was always maintained. As a result of this difference of temperature it is evident that vapour will rise from the hotter soil underneath into the colder air above, and some of it will be trapped by coming into contact with the cold grass.

While the experiments were being conducted on grass land, parallel observations were made on bare soil. Over soil the inverted trays collected more dew inside them than those over grass. A small area of soil was spread over a shallow pan, and after being weighed was exposed at the place where the soil had been taken out, to see if bare soil as well as grass lost weight during dewy nights. The result was that on all nights on which the tests were made the soil lost weight, and lost very nearly the same amount as the grass land.

Another method employed of testing whether vapour is rising from bare soil, or is being condensed upon it, consists in placing on the soil, and in good contact with it, small pieces of black mirror, or any substance having a surface that shows dewing easily. In this way small areas of the surface of the earth are converted into hygroscopes, and these test-surfaces tell us whether the ground where they are placed is cooled to the dew-point or not. So long as they remain clear and undewed, the surface of the soil is hotter than the dew-point, and vapour is being given off, while if they get dewed, the soil will also be condensing vapour. On all nights observed, these test-surfaces kept undewed, and showed the soil to be always giving off vapour.

All these different methods of testing point to the conclusion that during dewy nights, in this climate, vapour is constantly being given off from grass land, and almost always from bare soil; that the tide of vapour almost always sets outwards from the earth, and but rarely ebbs, save after being condensed to cloud and rain, or on those rarer occasions on which, after the earth has got greatly cooled, a warm moist air blows over it.

It seems probable that when the radiation is strong, that soil, especially if it is loose and not in good heat-communication with the ground, will get cooled below the dew-point, and have vapour condensed upon it. On some occasions the soil certainly got wetter on the surface during the night, but the question still remains, Whence the vapour? Came it from the air, or from the soil underneath? The latter seems the more probable source: the vapour rising from the hot soil underneath will be trapped by the cold surface-soil, in the same way as it is trapped by grass over grass land. During frost, opportunities are afforded of studying this point in a satisfactory manner, as the trapped vapour keeps its place where it is condensed. On these occasions the *under sides* of the clods, at the surface of the soil, are found to be thickly covered with hoar-frost, while there is little on their *upper or exposed surfaces*, showing that the vapour condensed on the surface-soil has come from below.

The next division of the subject is on dew on roads. It is generally said that dew forms copiously on grass, while none is deposited on roads, because grass is a good radiator and cools quicker, and cools more, than the surface of a road. It is shown that the above statement is wrong, and that dew really does form abundantly on roads, and that the reason it has not been observed is that it has not been sought for at the correct place. If a road is examined on a dewy night, and the gravel turned up, the under sides of the stones are found to be dripping wet.

For studying the formation of dew on roads, slates were found to be useful. One slate was placed over a gravelly part of the road, and another over a hard dry part. Examined on dewy nights the under sides of these slates were always found to be dripping wet, while their upper surfaces, and the ground all round, were quite dry.

The importance of the heat communicated from the ground may be illustrated by a simple experiment with two slates or two iron weights, one of them being placed on the ground, either on grass or on bare soil, and the other elevated a few inches above the surface. The exposed surface of the one resting on the ground, and in heat-communication with it, is found always to keep dry on dewy nights, whereas the elevated one gets dewed all over.

The effect of wind in preventing the formation of dew is referred to. It is shown that, in addition to the other ways already known, wind hinders the formation of dew by preventing an accumulation of moist air near the surface of the ground.

From an examination of the different forms of vegetation made on dewy nights it was soon evident that something else than radiation and condensation was at work to produce the varied appearances then seen on plants. Some kinds of plants were found to be wet, while others of a different kind, and growing close to them, were dry, and even on the same plant some branches were wet, whilst others were dry. The examination of the leaf of a broccoli plant showed better than any other that the wetting was not what we might expect if it were dew. The surface of the leaf was not wet all over, and the amount of deposit on any part had no relation to its exposure to radiation, or access to moist air; but the moisture was collected in little drops, placed at short distances apart, along the very edge of the leaf. Further examination showed that the position of these drops had a close relation to the structure of the leaf; they were all placed at the points where the veins in the leaf came to the outer edge, at once suggesting that these veins were the channels through which the liquid had been expelled. An examination of grass revealed a similar condition of matters: the moisture was not equally distributed over the blade, but was in drops attached to the tips of some of the blades. It therefore seemed probable that these drops, seen on vegetation on dewy nights, were not dew at all, but were an effect of the vitality of the plant.

It is pointed out that the excretion of drops of liquid by plants is no new discovery, as it has been long well known, and the experiments of Dr Moll on this subject are referred to; but what seems strange is that the relation of it to dew does not seem to have been recognised.

Some experiments were therefore made on this subject in its relation to dew. Leaves of plants that had been seen to be wet on dewy nights were experimented on. They were connected by means of an india-rubber tube with a head of water of about 1 metre, and the leaf surrounded with saturated air. All were found to exude a watery liquid after being subjected to pressure for some hours, and a broccoli leaf got studded all along its edge with drops, and presented exactly the same appearance it did on dewy nights. A stem of grass was also found to exude at the tips of one or two blades when pressure was applied.

The question as to whether these drops are really exuded by the plant by internal pressure, as in the above experiments, or are produced in some other way, is then considered. The tip of a blade of grass was put under conditions in which it could not extract moisture from the surrounding air, and, as the drop grew as rapidly under these conditions as did those on the unprotected blades, it is concluded that these drops are really exuded by the plant. Grass was found to get "dewed" in air not quite saturated.

On many nights no true dew is formed, and nothing but these exuded drops appear on the grass; and on all nights when vegetation is active, these drops appear before the true dew, and if the radiation is strong enough and the supply of vapour sufficient, true dew makes its appearance, and now the plants get equally wet all over, in the same manner as dead matter. The difference between true dew on grass, and these exuded drops, can be detected at a glance. The drops are always exuded at a point near the tip of the blade, and form a drop of some size, while true dew is distributed all over the blade. The exuded liquid forms a large diamond-like drop, while the dew coats the blade with a pearly lustre.

Towards the end of the paper the radiating powers of different surfaces at night are considered. By means of the radiation-thermometer, described by the author in a previous paper, the radiating powers of different surfaces were compared. Black and white cloths were found to radiate equally well: soil and grass were also almost exactly equal to each other. Lamp-black was equal to whiting. Sulphur was about 2/3rds of black paint, and polished tin about 1/7th of black paint. Snow in the shade on a bright day was at midday 7° colder than the air, while a black surface at the same time was only 4° colder. This difference diminished as the sun got lower, and at night both radiated almost equally well. In the concluding pages of the paper some less important subjects are considered.

5. Observations on the Structure of *Lumbricus complanatus*, Dugès. By Frank E. Beddard, M.A., F.R.S.E., F.Z.S., Prosector to the Zoological Society of London.

A very considerable number of earthworms have been referred to the genus *Lumbricus*, and its immediate allies *Dendrobæna*, *Allolobophora*, &c., which have in reality no marked affinities with any of these genera. Such are, for example, *Lumbricus corethrurus* of Fritz Müller, which Perrier has shown to belong to a distinct generic type described by him under the name of *Urochæta*, and *Lumbricus microchæta* of Rapp, which is also the type of a new genus, widely differing from *Lumbricus* proper. On the other hand, a very large number of species have been referred to the latter genus, concerning which there is little or no knowledge, so that it is impossible to speak with any certainty as to their exact systematic position.

No less than fifty-nine such species are enumerated by Vejdovsky, in his lately-published *System und Morphologie der Oligochæten*, some of which, however, are evidently members of the genus *Lumbricus*, or its immediate allies. In the memoir referred to, which is not merely a record of new observations, but a thoroughly digested summary of previous work upon the subject, Dr Vejdovsky only allows three species that can be at present unhesitatingly assigned to the genus *Lumbricus;* of the other *Lumbrici*, eighteen species belong to Eisen's genera *Tetragonurus*, *Allolobophora*, *Allurus*, and *Dendrobæna;* twenty-eight probably belong to one of the four genera enumerated, while thirty-two may or may not (in some cases, *e.g.*, *L. microchæta* certainly not) belong to any of these genera; they are put under the head of "species inquirendæ." It is with one of these latter that the present paper deals. I do not, however, wish to identify the species that I am about to describe with *Lumbricus complanatus* of Dugès without a certain reservation. Absolutely nothing is known about the structure of by far the majority of the species of *Lumbricus;* and it is evidently therefore a matter of impossibility to determine its systematic position with any pretence to accuracy.

The following observations rest upon the examination of a single specimen, which I owe to the kindness of Professor A. G. Bourne; the specimen was brought from Naples, and has been excellently preserved by means of perchloride of mercury.

One of the earliest writers on the structure of earthworms—Dugès—has left a valuable paper* on the anatomy and systematic characters of those species of *Lumbricus* which he was able to obtain in France. He distinguishes six separate species of *Lumbricus* as occurring in that country; but it is at present quite impossible to state how far his systematic determinations are of value. The whole subject wants to be gone into afresh with the help of anatomy, and until that is done there can be no real attempt to classify the species of *Lumbricus*, as well as to decide how far the generic characters adopted by Eisen are of value.

Of the species described by Dugès two appear, so far as I can judge, to approach very nearly the species described in the present paper. These are *Lumbricus gigas* and *L. complanatus.* The structure of both species is very closely similar to that of my own species, but the external characters of the latter correspond more with those of *L. complanatus.*

The anatomy of *L. gigas* is described in some detail, and the descriptions are illustrated by a number of figures. There are, of course, numerous statements in the paper which require correction in the light of more recent knowledge; the spermathecæ are spoken of as testes, and the testes themselves, or rather the vesiculæ, are generally denominated ovaries, and their ducts oviducts. The canal which Dugès figures as connecting the spermathecæ has not, as far as modern investigations go, any existence. Another figure illustrates with sufficient accuracy the two vasa deferentia funnels, their ducts uniting into a common vas deferens; the "testes" are shown as being in continuity with the margins of the funnels, so that it is evident that in this species, as in others of *Lumbricus*, the testicular sacs are not the true testes, but merely a mass of spermatozoa in various stages of development, enclosed in a sac-like structure which is simply an outgrowth of the margins of the vas deferens funnel.

The main point of interest in connection with these figures is the presence of four distinct vesiculæ seminales, and no less than seven copulatory pouches on each side of the body.

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* *Annales d. Sciences Nat.*, t. xv. (1828), p. 286, pls. viii., ix.

It is well known that in the common British *Lumbricus* there are only three pairs of vesiculæ seminales, and two pairs of small spherical copulatory pouches, which are found in the same segments. The only exceptions to this rule mentioned by Vejdovsky, apart from the present species, are *Allolobophora chlorotica*, which has three pairs of copulatory pouches (according to Dugès), and *Dendrobœna*, which has only one pair. Dugès mentions that seven pairs of copulatory pouches are not invariably characteristic of *L. complanatus*. He says:—"Ces séries n'ont pas toujours la même étendue, puisque le nombre des vésicules varie de deux à sept pour chaque côté. Ces différences sont-elles spécifiques, comme le pense M. Savigny? Je crois plutôt qu'il faut les rapporter au temps où l'on en fait l'étude: en effet, à mesure que l'époque de l'accouplement s'éloigne, le volume de ces organes diminue; les plus extrêmes, aux deux bouts de la série, s'atrophient les premiers, et il arrive une époque où l'on n'aperçoit qu'avec peine les rudiments de ces parties si saillantes en un autre temps."*

It appears to me that the latter supposition is the correct one, and that the greater number of copulatory pouches is in all probability the typical number in fully-developed specimens.

I have recently had the opportunity of confirming Dugès' supposition in a species of *Perichæta* (probably *P. affinis*). My friend Mr H. E. Barwell, of Manila, kindly forwarded me a large number of earthworms from that locality at my request, and among them were numerous individuals of this *Perichæta*, in which the copulatory pouches varied in the most extraordinary way in number, though not, as far as I could ascertain, in structure. There were no other differences in structure between the different individuals.

On the other hand, no individual of the British species of *Lumbricus* has, so far as I am aware, ever been recorded which possesses more than two pairs of copulatory pouches. The number of copulatory pouches present may serve as an indication of specific rank, but it is necessary to make use of this character with great caution. In the present instance it may be taken for granted that seven separate copulatory pouches distinguish *L. gigas* from most other species of the genus.

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* *Loc. cit.*, p. 327.

A subsequent paper by Dugès* considerably augments the number of species. In that paper (p. 23) the author states that *L. complanatus* agrees with *L. gigas* in possessing seven pairs of copulatory pouches and four pairs of vesiculæ seminales. For this reason I am inclined for the present to regard the species to be described in the present note as identical with *L. complanatus;* it differs from *L. gigas* as described by Dugès (1) in the distribution of the setæ, and (2) in the position of the clitellum. In *L. gigas* the setæ of the two pairs are closely approximated together, while in *L. complanatus* they are rather more widely separated. The arrangement of the setæ in *L. complanatus* is figured in the first of the two memoirs referred to, and corresponds, so far as I can make out, exactly with the species studied by myself; the two setæ of the ventral pairs are rather further apart than those of the dorsal pair. The clitellum in my species extended from segments 28–38, occupying therefore 10 segments. Dugès states that in *L. complanatus* the clitellum extends from 29–39, which, as he counts the buccal lobe as a distinct segment, is precisely the same; in *L. gigas*, on the other hand, the clitellum occupies 22 rings, ending with the 53rd.

In Hoffmeister's *Familien der Regenwürmer* three species are mentioned in which the clitellum occupies *about* the same segments as in *L. complanatus*, *L. stagnalis* (apparently identical with *L. planatus*) is one species; the other two are *L. agricola* (= *L. terrestris*, *Enterion herculeum*) and *L. riparius* (= *L. octædrus*, *chloroticus*, *virescens*, *anatomicus*); in all these, however, the setæ of each pair are very closely approximated, and, moreover, except in *L. agricola*, the segments contained in the clitellum are not more than nine; so, at any rate, does it appear from Dugès' table of classification of the species in the second of his two memoirs referred to.

Dugès does not give an elaborate account of the generative apparatus in *L. complanatus*, merely remarking that there are four seminal vesicles and seven pairs of copulatory pouches.

In the specimen before me there are four seminal vesicles on each side of the body, occupying apparently segments 8–11; they are approximately of equal size and rounded in shape, firmly attached to the mesentery dividing these segments. The copulatory pouches have an arrangement rather different from that figured by Dugès in

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* *Loc. cit.*, t. xv. pl. ix. fig. 25.

*L. gigas.** He gives no figure of those structures in *L. complanatus*, only mentioning that they are four to seven pairs in number. In the individual dissected by myself the copulatory pouches are disposed as shown in the accompanying figure (fig. 1). In the fifth, sixth, and seventh segments are a pair of spherical pouches, one to each segment, furnished with a very short duct, which perforates the body-wall on a line with and behind the innermost seta of the dorsal pair; in the eighth segment the pouch, on the left side of the body, opens in the posterior region of the segment in common with a small diverticulum lying in the segment behind; on the right side of the body the condition of this pouch is exactly reversed, the small diverticulum lying in the eighth segment and the larger portion of the pouch in the ninth; in the ninth segment, in addition to the

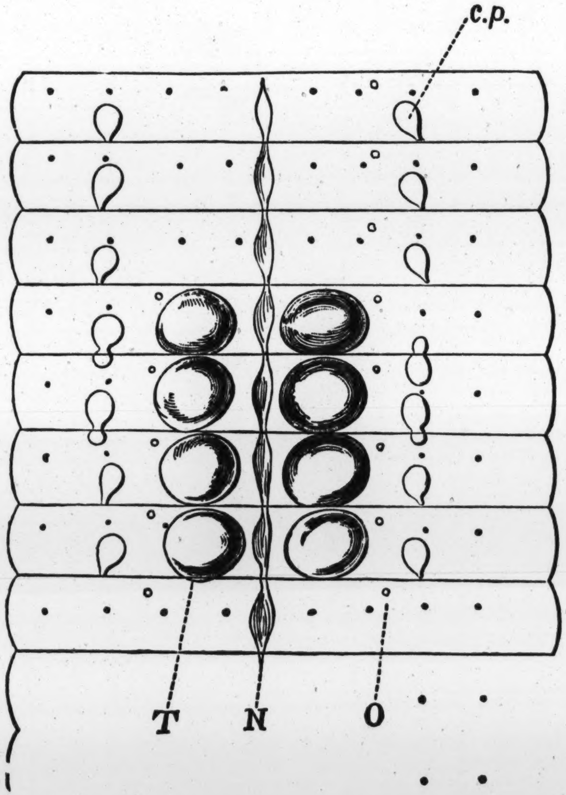

FIG. 1.—Diagram of the genital region of *Lumbricus complanatus.* T, vesiculæ seminales; *c. p.*, spermathecæ; N, nerve cord; O, orifices of nephridia. The setæ are indicated by black dots.

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* *Tom. cit.*, . ix. fig. 1.

diverticula just mentioned, is a large pouch in the posterior region of the segment, connected with a small diverticulum in the following segment; in segments ten and eleven were a pair of pouches lying close to the posterior mesentery, and apparently unprovided with any diverticulum. The pouches were in every case fully distended with the contained semen. I also studied the series of copulatory pouches of one side of the body (the right) by means of longitudinal sections, which revealed the position of the external apertures not visible on an inspection of the intact worm. These apertures are situated at the anterior extremity of each segment, in the groove which separates it from the segment in front and on a line with the innermost seta of the more dorsally placed pair. It has been already mentioned that the copulatory pouches themselves lie in most instances in the posterior region of their segments, in close contact with its posterior wall. The duct of each pouch, which is extremely narrow, perforates the body wall just at the point where the mesenteries arise, and, running obliquely backwards, opens on to the exterior in the place already mentioned, which corresponds with segment *behind* that which contains the pouch itself.

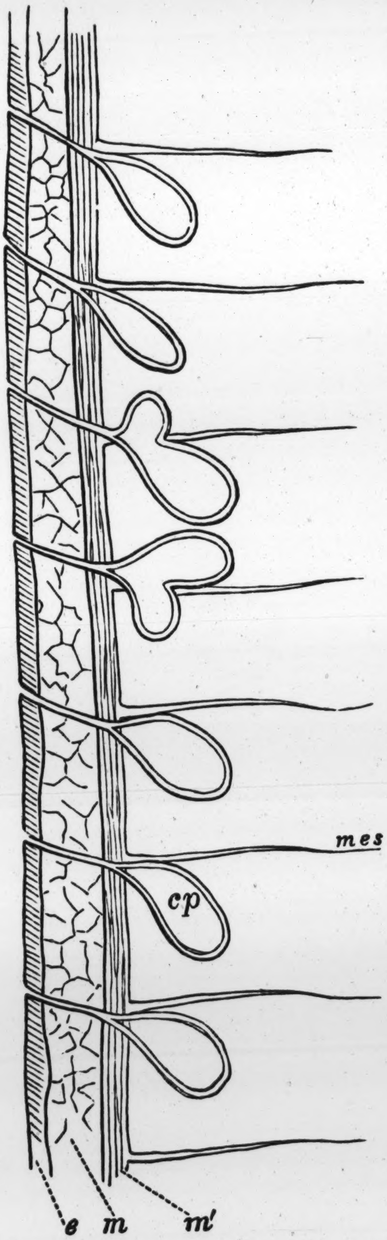

FIG. 2.—Diagram of longitudinal section through spermathecæ of right side of body. The spermatheca at the top of the figure is the last, *i.e.*, that occupying segment 11; *mes*, mesenteries; *c p*, spermatheca; *e*, epidermis; *m*, transverse muscular layer; *m'*, longitudinal muscular layer.

This fact is illustrated by the accompanying woodcut (fig. 2), which represents a vertical section through the seven pouches of the right-hand side of the body. In segments 8 and 9, it has already been mentioned that the pouches are not contained in a single segment, but are furnished with a diverticulum which passes through the mesentery into the segment behind. The series of sections confirm these statements, and also show that the duct of the copulatory pouch arises from about the junction of its two halves, which are only separated from each other by a slight constriction at the mesentery; as diverticula, in fact, they are not well marked, the structure being identical throughout.

In every case the interior of the spermatheca was filled with a mass of spermatozoa.

I am able therefore, generally speaking, to confirm the accuracy of Dugès' observations on the copulatory pouches of *Lumbricus complanatus*, and such a confirmation is evidently necessary, since it is often stated, when dealing with the structure of *Lumbricidæ*, that *Lumbricus* differs from other genera in the fact that the copulatory pouches are contained in the same segments as the testes, and are only two or three pairs in number. The present species combines the characters of most *Lumbrici* on the one hand, and such genera as *Pontodrilus*, *Urochæta*, &c., on the other, in so far that the copulatory pouches are present both in the segments which contain the testes and in certain of those which precede them. The number of pairs also is larger than is met with in any other known earthworm.

Another fact of interest is the presence of diverticula, which, characteristic of many other genera, *Perichæta*, &c., have not to my knowledge been described in *Lumbricus;* they have apparently escaped the attention of Dugès, always supposing that his species is the same as that described in the present paper. It is rather difficult, however, in the present species to say which part should be regarded as the copulatory pouch itself and which the diverticulum. In segment nine, for example, the pouch on the right-hand side of the body was considerably larger than its "diverticulum," and on the left-hand side smaller. In *Perichæta* there is never any such difficulty, for the diverticula have a very different appearance; they are usually different in shape from the copulatory pouch; and in a species of *Acanthodrilus*, I have pointed out that there is also a difference in minute structure* between the two portions of the copulatory pouch. In *Lumbricus complanatus* there is no difference in shape; only a difference in size, and the structure is identical. This matter is a little more important than might appear at first sight, because if we are to allow that the two large pouches in segment nine are each the equivalent of a copulatory pouch, we shall have the apparent anomaly of two pairs of copulatory pouches in the same segment. It is generally believed that the segment which contains the gizzard is in reality the equivalent of two segments which have become fused through the abortion of the intervening septum, possibly of mechanical advantage to the gizzard, as rendering its movements more free. In *Perichæta affinis*, Perrer records† the presence of two pairs of copulatory pouches in the gizzard "segment," besides two rings of setæ. I have observed the same thing in other species of this genus. Is it legitimate to suppose that in *L. complanatus* we have a record of the former presence of the gizzard in the same region of the body where it now occurs in *Perichæta*, though not in any *Lumbricus*?

The supposition of an aborted mesentery and a consequent fusion of two segments is not, however, so trustworthy in this species as in *Perichæta*, because there is certainly only one nephridium and one row of setæ as in all the adjacent segments, and so, perhaps, it is better to suppose that the copulatory pouch of segment 8 has a very large diverticulum,—larger than the pouch itself,—a more normal arrangement being found on the left side of the body. A third alternative is possible, viz., that more than a single pair of copulatory pouches may be present in the same segment. As an evident abnormality, I have observed a case of this kind in a specimen of *Perichæta affinis*—the species which has already been referred to above as affording an instance of variability in the number of the copulatory pouches in different individuals of the same species; in one specimen of this worm there were three copulatory pouches in one segment placed in a row. An inspection of fig. 2 would appear at first sight to prove that the second alternative mentioned above is the correct one. In every case it will be noticed that the duct of the pouch passes backwards to open on to the exterior at a point opposite the

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* *Proc. Zool. Soc.*, 1885, p. 829.

† *Nouv. Arch. d. Mus.*, t. viii. p. 111.

segment behind that which contains the pouch. In this way we must assume, for the sake of making all the pouches of the series to correspond, that the minute portion of the pouch which lies in segment 8 is the pouch itself, and not a diverticulum. On the other hand, perhaps, the fourth and fifth pouches still retain the characters that are met with in most species of *Lumbricus*,* where the pouch is situated near to the anterior region of its segment, and the duct opens on to the body-wall in the groove between this segment and the one in front, with this difference, that a part of the pouch has grown through the wall and forms a kind of diverticulum; if the diverticulum continues to grow at the expense of the pouch until the latter vanishes altogether, we arrive at the condition which is characteristic of certain other *Lumbrici;* in any case, the two pouches of segments 8 and 9 appear to be an indication of a connection between the unusual disposition of the pouches in the present species and their more usual arrangement in *Lumbricus* generally, and in other earthworms (*i.e.*, situated in the anterior region of the segment, and opening in the groove between this segment and the one in front).

I have, therefore, been able to show in the present paper that a

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* This is not, however, invariably the case. In some individuals the spermathecæ are, as in the present species, placed near to the posterior wall of the segment which contains them; in other individuals, possibly different species, the spermatheca, as in the majority of earthworms, is placed nearer to the anterior wall of the segment. This may prove to be a difference of specific importance. I find that this variation exists in the figures of other observers. Lankester (*Quart. Jour. Micr. Sci.*, 1865) figures the two spermathecæ as lying by the anterior wall of their segments (9 and 10), and opening in the groove between each of those segments and the one in front. On the other hand, Hering (*Zeitsch. f. Wiss. Zool.*, 1857) states that the spermathecæ are situated in 9 and 10, and open between 9–10 and 10–11. D'Udekem (*Mem. Cour. d. l. Acad. roy. de Brux.*, 1856) figures and describes a disposition of the spermathecæ identical with that observed by Hering.

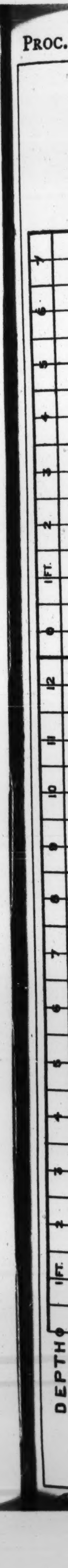
Note added May 15, 1886.—A paper by Dr Rosa, "I lumbricidi del Piemonte," which I had overlooked in writing the above, has come into my hands through the kindness of the author. *L. complanatus* is described in this paper under the name of *Allolobophora complanata;* the author, however, does not mention the diverticula of the spermathecæ, nor is there any indication of them in his figure; if the presence of these should ultimately prove to be a specific character, I would propose to name my species *Lumbricus* (*Allolophora*) *rosæ*, after the Italian naturalist. I notice also that in this paper the position of the spermathecæ in the anterior or posterior region of the segment is used as a specific character. The specimen described in the present paper shows how the change of position of the spermathecæ is brought about.

certain species of earthworm, characterised by the fact that the clitellum occupies 10 segments ending with the 38th, that the setæ of each pair are rather widely separated, the nephridial pores being placed in front of and a little to the outside of the outermost of the two ventral setæ, has a certain internal structure, the most marked characters being (1) the presence of four distinct vesiculæ seminales, (2) the presence of seven distinct pairs of spermathecæ situated in segments 5–11 inclusive. It is only by such comparisons of internal and external structure that the limits of genera and species can be determined, and the at present confused synonymy of *Lumbricus* reduced to anything like order.

6. On the Physical Conditions of Rivers entering a Tidal Sea; from Observations on the Spey. By Hugh Robert Mill, B.Sc., and T. Morton Ritchie, B.Sc. (Plates XIII., XIV., XV.)

In connection with the observations carried on by one of us on the salinity and temperature of estuaries,* it seemed desirable to examine the condition of the water near the mouth of a river flowing directly into the sea. For this purpose it was necessary to select a stream of considerable volume and free from pollution. As the Spey appeared to be the most suitable river, we spent the month of August at Garmouth, a small town close to the old port of Kingston, on the west side of the river mouth. We have to express our thanks to Mr Balmer, Commissioner at Fochabers to the Duke of Richmond and Gordon, for much kindly interest in our work, and for affording us the use of the facilities presented by the salmon-fishing station of Tugnet on the east bank of the river.

The Spey is the most rapid river in Scotland, and next in size to the Tay. It is 120 miles long, including windings, and drains an area of 1245 square miles. It flows in a northerly direction into the Moray Firth, which is not an estuary in any sense of the word, but



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* Mill, "On the Salinity of the Water in the Firth of Forth," *Proc. Roy. Soc. Edin.*, xiii. pp. 29–64; Abstract in *Nature*, xxxi. p. 541; "On the Temperature of the Water in the Firth of Forth," *Proc. Roy. Soc. Edin.*, xiii. pp. 157–167; "On the Salinity of the Estuary of the Tay and of St Andrews Bay," *ibid.*, pp. 347–350; "On the Physical Conditions of Water in Estuaries," *Brit. Ass. Rep.*, 1885, and *Scottish Geographical Magazine*, ii. pp. 20–26.

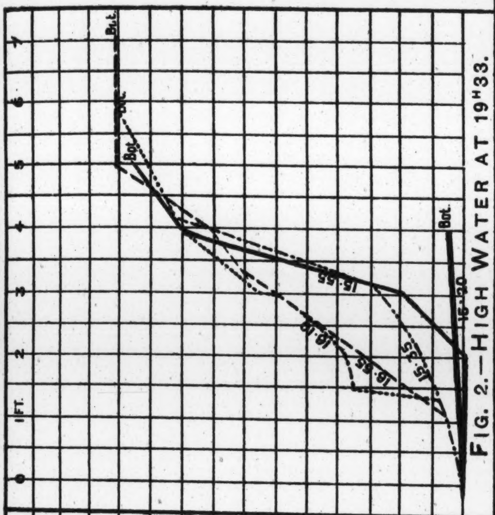

FIG. 2.—HIGH WATER AT 19^H33.

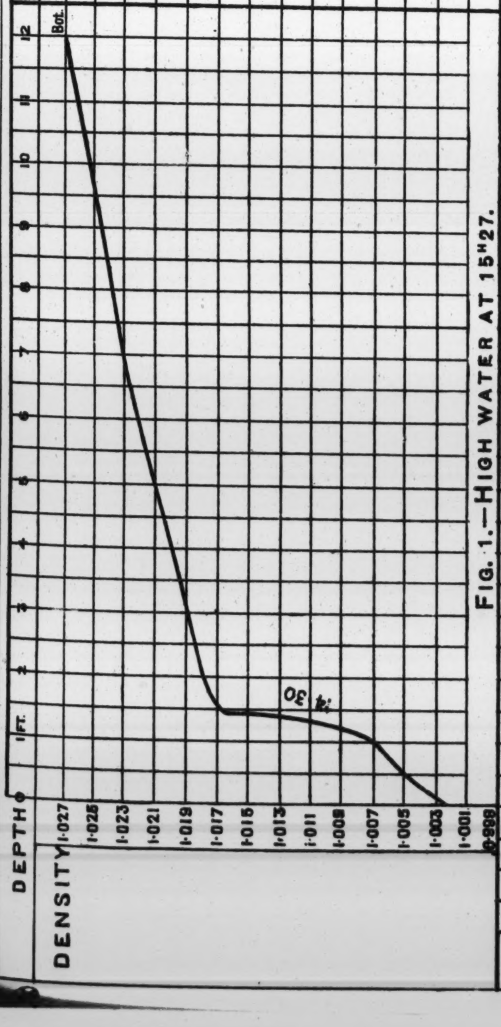

FIG. 1.—HIGH WATER AT 15^H27.

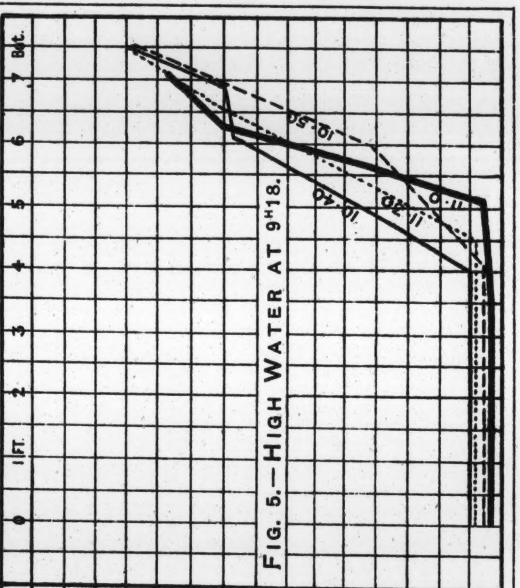

FIG. 5.—HIGH WATER AT 9^H18.

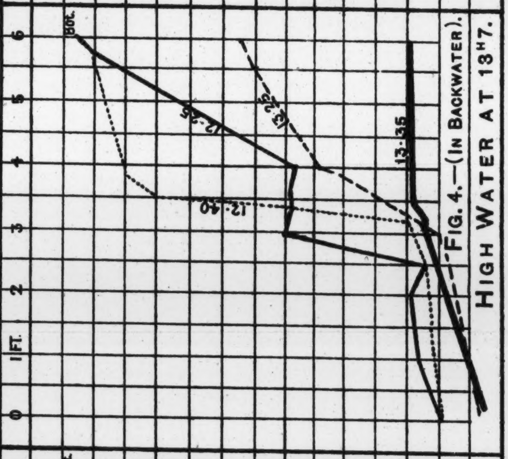

FIG. 4.—(IN BACKWATER).
HIGH WATER AT 13^H7.

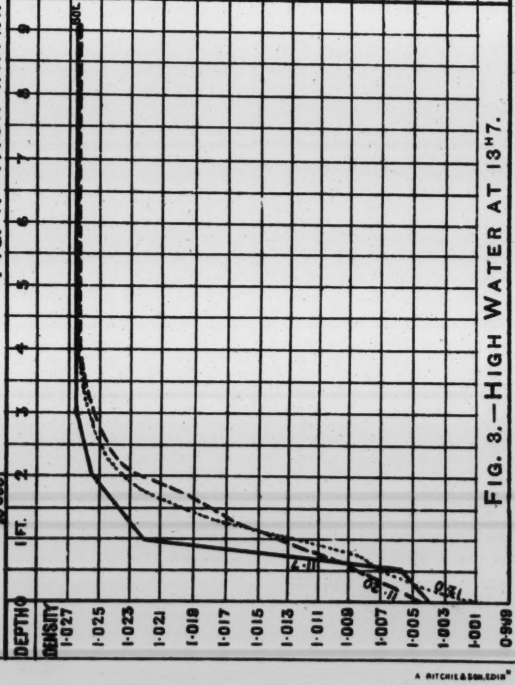

FIG. 3.—HIGH WATER AT 13^H7.

A. RITCHIE & SON, EDIN^R.

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RIVER SPEY—VARIATION OF SALINITY WITH DEPTH AND TIME.

RIVER SPEY—VARIATION OF SALINITY WITH DEPTH AND TIME.

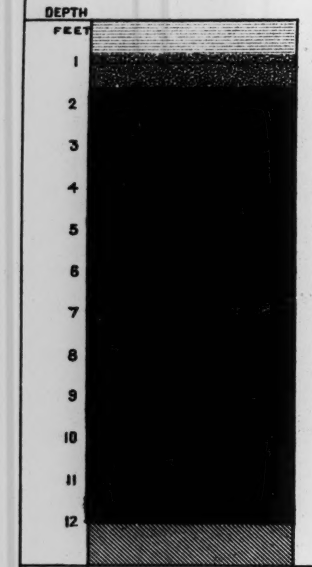

Fig. 1.—5^hrs Flood

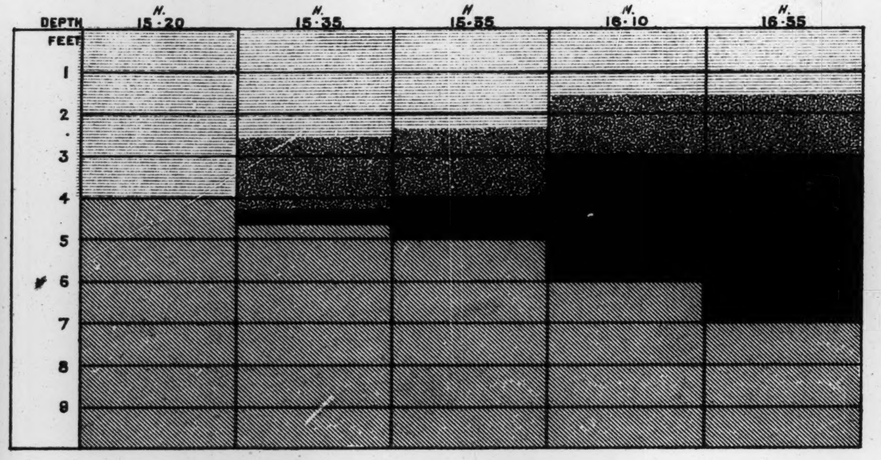

Fig. 2.—High Tide at 19^H33.

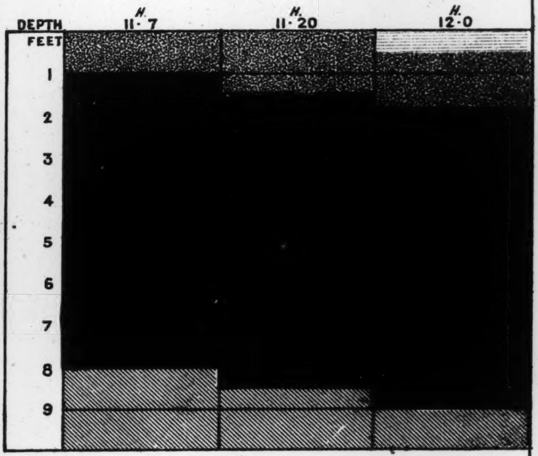

Fig. 3.—High Tide at 13^H7.

Percentage of Salt present.

(Index of Colours)

Under 0·5% (up to 10% of seawater)	▤
Over 0·5% and under 2%	▒
Over 2% (more than 60% seawater)	■

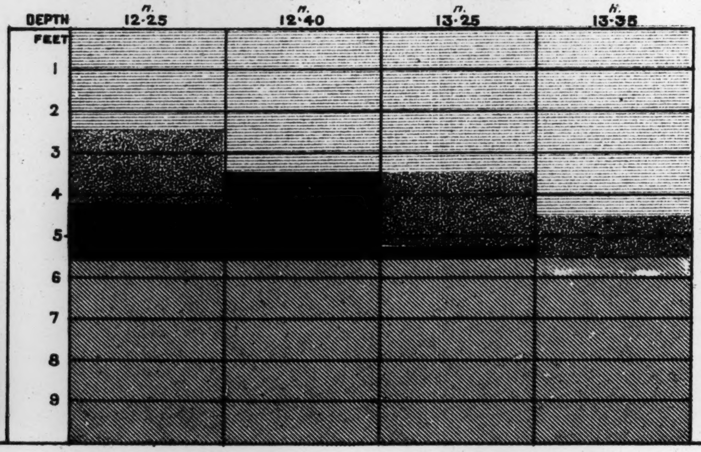

Fig. 4.—(Backwater). High Tide at 13^H7.

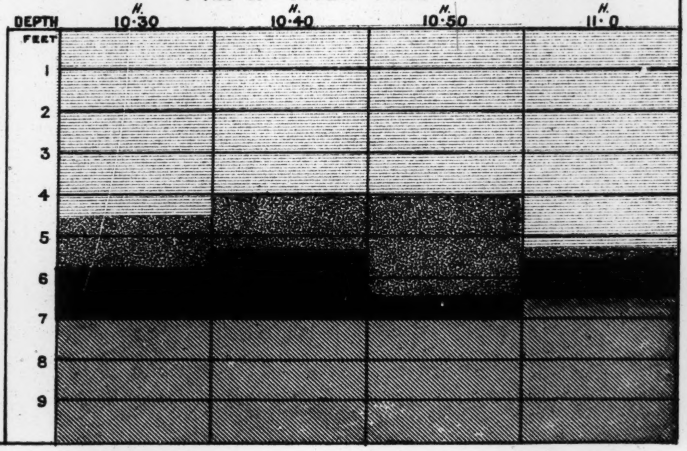

Fig. 5.—High Tide at 9^H18.

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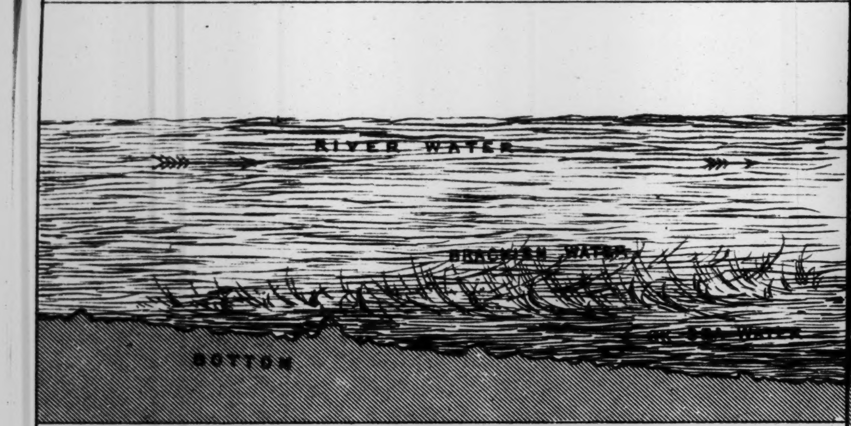

FIG. 1.—SUPPOSED FLOW OF TIDE INTO RIVER SPEY.

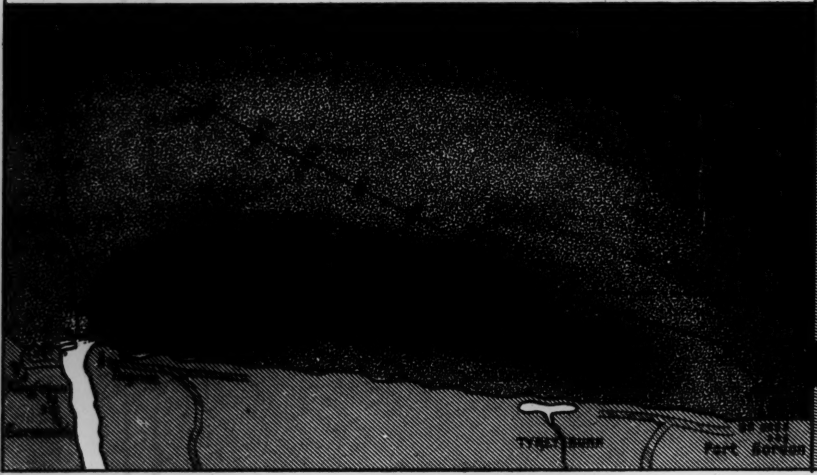

FIG. 2.—SALINITY OF SPEY BAY, 18TH AUGUST 1885.

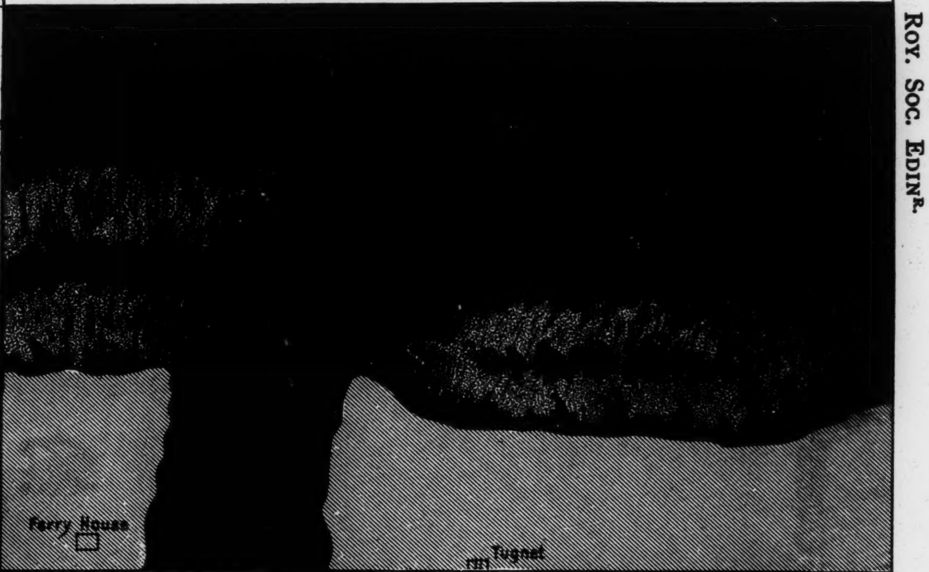

FIG. 3.—COURSE OF SPEY WATER IN THE BAY, 13TH AUGUST 1885

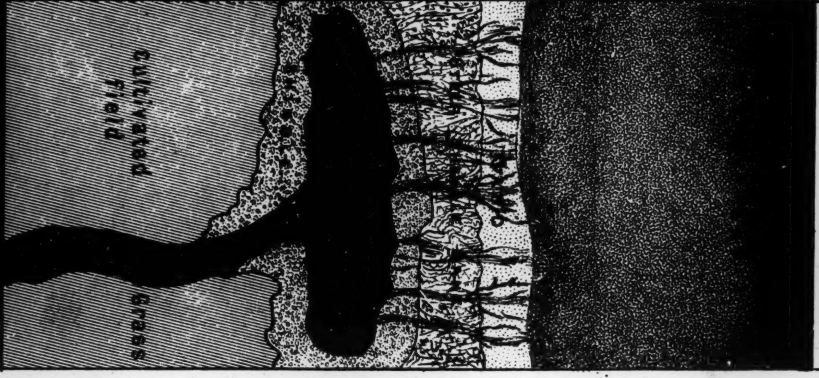

FIG. 4.—OUTLET OF TYNET BURN

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an inlet of the sea, and as such possessed of the distinctive marine characteristics. As a salmon river, it ranks next to the Tay and the Tweed. It is particularly noted for the suddenness and intensity of its floods, and for the variable nature of the channel, the stream being frequently diverted in a new direction. Twenty-five years ago a channel was cut to turn the river from the village of Kingston, many of the houses of which had been undermined and carried away. One result was the destruction of the harbour, so that now a boat can enter or leave the river only in the calmest weather, and then at high tide. This fact added considerably to the difficulties of work in the bay, and prevented it from being so complete as we desired. The bay is very shallow near the shore, and the depth is yearly diminishing on account of the stones and silt brought down by the river. The beach on each side for several miles is covered with fields of river-borne shingle massed in wave-like ridges parallel to the coast line, and hardly touched by vegetation.*

The town of Lossiemouth lies 7½ miles to the west of the Spey, and Buckie 5 miles to the east. Halfway to Buckie, 2½ miles east of Tugnet, is the fishing village of Port Gordon, the nearest harbour to the Spey available in ordinary weather.

The river near the mouth is confined to a narrow channel (250 feet wide at high water, and about 150 at low tide), and partly on this account and partly because of the slope of its bed, it is very rapid. The depth varies from 3 to 6 feet at low tide, and every flood effects some change in the position of the shingle banks and shoals. The western side of the river near the mouth is a shingle bank running into a line of low sandy islands; on the east there are simple shingle heaps forming a breakwater about 30 yards wide which separates the river from the sea. Just at the mouth a ridge of shingle, entirely bare at low tide, runs in an east-north-easterly direction, and sends the river current out to sea towards the east: its prolongation forms a bar across the channel with not more than 2 feet of water on it. Since the bar and banks are constantly shifting in size, shape, and position, all observations which depend in any way on the form of the river mouth must be considered as applicable only to the time when they were made.

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* Mr Balmer informs us that the river was diverted in February 1860 to a straight course, in the expectation that by its action on the shingle banks it would gradually move back to its old westerly position. Instead of doing so, it cut a new channel more to the east, and continued for several months to extend this until the mouth had advanced about quarter of a mile to the eastward. Then the normal action recommenced, and shingle has been steadily laid down on the east bank and cut off from the western ever since, so that the mouth is moving westward every month; and in a few years it is expected to come back to the position it occupied prior to 1860.

The salmon-fishing station of Tugnet stands on the east shore about one-third of a mile from the river mouth. A backwater separated, except at high tide, from the main stream by a sandy flat, runs up from near the river mouth to Tugnet. On the west side a narrow shallow channel, separated by a low grassy island from the river, comes down past Garmouth, and at its termination a burn which trickles across the mud of Kingston Basin at low tide, enters the river. At high tide this burn becomes a passage with considerable depth of water, leading to a broad expanse below the houses at Kingston, and separated from the sea by a high shingle bank. The relative positions of these parts of the river may be understood from the accompanying sketch (fig. 1), which is drawn roughly from memory, and is not to scale.

Even when the river is small and the tide full the water is fresh on the surface.* The rising tide diminishes the velocity of the current, both by damming back the fresh water and by widening the connection with the sea, and so letting it spread over a larger area.

The conditions of working in a small coble in a rapid stream of slight depth, in which considerable variations of salinity might be expected, necessitated some modifications of the usual modes of obtaining and examining samples of water. The slip water-bottle was useless as a collector, because it enclosed a vertical column of water a foot high, and it soon became apparent that very considerable changes of density took place in a distance of 3 inches. It was found best in the circumstances to use an ordinary stoppered bottle (of about 1·5 litre capacity) lashed to a pole or attached to a sounding-line which was heavily weighted,† and to draw out the stopper by a cord when the desired depth was reached. The pole or line was graduated to feet. By this means, the bottle being lowered full of air, water was let in from a plane that could be determined to 1 inch in calm weather. It was a little difficult to get a true bottom sample in this way; but it was secured, when a

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* The fact that sea water forced its way up the bed of a tidal river under the opposing current of fresh water has been known at any rate since 1812, when the phenomenon was studied on the Dee at Aberdeen by Mr Robert Stevenson.—David Stevenson, *Canal and River Engineering*, 2nd ed., p. 124.

† The *hydrophore* of Mr Robert Stevenson (*Canal and River Engineering*, p. 126) would have been more suitable, but there was no opportunity of getting one made.

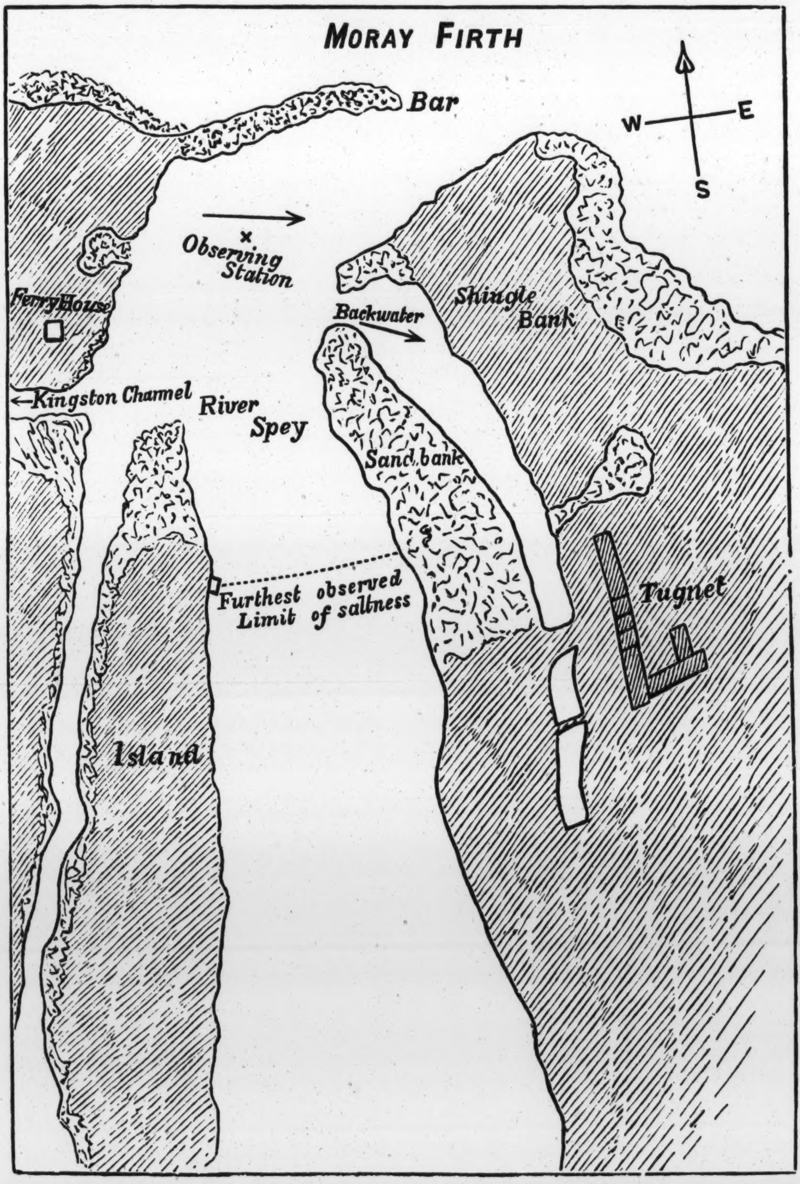

FIG. 1.—Sketch-Map of Mouth of Spey, August 1885. Light shading shows parts covered at high water.

sounding-line was used, by fixing a weight to the neck of the bottle and paying out line enough to let it lie flat.

It was impracticable, on account of the time required, to use the delicate hydrometer for determining the density of the hundreds of samples taken in tracing the mixture of sea and river water; and we were obliged to be content with occasionally reserving a bottom, intermediate, and surface sample for careful determination, while a small directly-graduated hydrometer was made use of in the boat. This hydrometer was marked to read "specific gravities" from 1·000 to 1·030 at 60° F., but since only an indication of the salinity was required the temperature, which averaged about 58° and which often varied 1° or even 2° between surface and bottom, was neglected.

Throughout this paper the density of water when given to the third or fourth decimal place (*e.g.*, 1·026 or 1·0255) is only roughly approximate, and is not corrected for temperature; the observations were made, as a rule, while the water had a temperature of from 12° to 16° C. When it is given to five places (*e.g.*, 1·02345) it is accurate to within 0·00005, and is calculated to 15°·56 C. whatever the original temperature of the sample was. The observed temperature of water and air is given in degrees Fahrenheit, in consistence with the usage of meteorologists. Instrumental errors are not allowed for, as the same thermometers were always used for air, surface-water, and deep-water temperatures respectively; the absolute uncertainty, which is a constant for each thermometer, does not exceed 0°·2 in any case. Time is reckoned from 0 hours, midnight, to 24 hours.

Observations on Tidal Variations of Salinity.

Numerous observations were made at a point about 100 yards inside the bar when tide was flowing, and some in the same position while it was ebbing. The mode of working was to anchor the coble in mid-stream, and, beginning at the surface, to take samples at every foot of depth, to observe the density with the small hydrometer, and where there was a sudden rise in density between any two specimens, to examine samples from every 3 inches in that foot. By this means the vertical distribution of salinity was traced. After a few minutes the process was repeated, and so on until the work had to be stopped by the ebb current becoming too strong for the anchor. It was sometimes found advisable to begin at the bottom and work upwards.

The following are the observations made in this way :—

I. *Flowing Tide.*—August 1, observations at 14^{h}.30; high water at 15^{h}.27. Depth 12 feet (spring tide). Temperature of water at surface 62°·5, at 6 feet 58°·7, and at bottom 55°·6.

Depth, ...	Surf.	8 in.	1 foot.	1 ft. 6 in.	3 feet.	6 feet.	Bot. 12 ft.
Density (rough), . . .	1·002	1·006	1·007	1·017	1·019	1·022	1·027
,, at 15°.56, .	1·00108	...	...	...	...	1·01668	1·02481

This, being a representative case, is shown by the curve, Plate XIII. fig. 1, and on the coloured diagram, Plate XIV. fig. 1, where the depth of colour is proportional to the amount of salt in solution at each position.

II. *Flowing Tide.*—August 3, observations at 15^{h}.25 and 16^{h}.0; high water at 16^{h}.56. Depth 8 feet. Weather bright and calm; heavy surf on the bar. Temperature of air 54°·5, surface water 60°·0, at 4 feet 58°·0, bottom 56°·7. A shower on the previous night might have raised the river very slightly.

At 15^{h}.25.

Depth, .	Surf.	2 ft.	3 ft.	4 ft.	5 ft.	6 ft.	7 ft.	Bot. 8 ft.
Density (rough), .	1·000	1·010	1·015	1·020	1·020	1·022	1·0235	1·0235
Density at 15°·56 C.	0·99972	...	...	1·01571	...	...	...	1·02162

At 16^{h}.0.

Depth,	Surf.	1 in.	3 in.	6 in.	9 in.	1 ft.	1 ft. 6 in.	2 ft.	3 ft.	6 ft.	Bot. 8 ft.
Density (rough), .	1·002	1·0025	1·002	1·003	1·003	1·0035	1·007	1·015	1·021	1·0235	1·0235
Density at 15°·56C.	1·00000	...	...	...	...	...	...	...	1·01382	...	1·02037

The water on the shore half a mile east of the river mouth had, at 14^{h}.0, the density 1·02420.

III. *Flowing Tide.*—August 5, observations from 14^{h}.15 to 16^{h}.55; high water at 19^{h}.33. Depth increasing from 4 feet to 7 feet. Heavy surf on the bar; no wind. Temperature of air 53°·0, of surface water (mean for the whole time) 57°·2, at half depth 56°·7, and at bottom 56°·4.

At 14^{h}.15. Surface 1·001, correctly 0·99927; bottom 1·002, correctly 0·99937.

Bottom density observed at the following hours, that at $15^h.30$ being the same for mid-stream and near each bank.

Hour, ...	$14^h.50$	$14^h.57$	$15^h.5$	$15^h.18$	$15^h.20$	$15^h.30$	$15^h.35$
Bottom density (rough), .	1·001	1·001	1·002	1·003	1·002	1·004	1·018

	Surf.	1 ft.	1 ft. 3.	1 ft. 6.	2 ft.	3 ft.	4 ft.	5 ft.	6 ft.	7 ft.
At $15^h.55$.										
Depth, ...	Surf.	1 ft.	1 ft. 3.	1 ft. 6.	2 ft.	3 ft.	4 ft.	5 ft.	6 ft.	7 ft.
Density (rough), .	1·0015	1·001	...	...	1·001	1·005	1·019	1·022	...	...
Density at 15°·56 C. .	0·99917	...	...	...		1·00600	...	1·01810	...	...
At $16^h.10$.										
Density (rough), .	1·001	1·001	1·002	1·008	1·008	1·014	1·019	1·017	1·022	...
At $16^h·55$.										
Density (rough), .	1·001	1·002	...	...	1·004	1·014	1·016	1·023	1·022	1·023
Density at 15°·56 C.	0·99944	...	...	...	...	...	1·02041	...	...	1·02258

These data are used in constructing the curves on Plate XIII. fig. 2, and the coloured diagram Plate XIV. fig. 2.

IV. *Flowing Tide.*—August 11, observations from $10^h.40$ to $11^h.40$; high water $13^h.7$. Very strong wind blowing down the river. Temperature at surface 57°·5. The high wind made it impossible to read the hydrometer with great accuracy.

At $10^h.40$. Depth, . .	Surf.	1 ft.	2 ft.	3 ft.	4 ft.	5 ft.	6 ft.	7 ft.	8 ft.	9 ft.
Density (rough), . .	...	...	...	1·026	1·025	1·0265	1·0265	...	1·027	1·028

	Surf.	3 in.	6 in.	9 in.	1 ft.	1 ft. 3.	1 ft. 6.	1 ft. 9.	2 ft.	3 ft.	4 ft.	9 ft. bot.
At $11^h.7$,	1·004	1·004	1·006	1·014	1·022	1·024	1·024	...	1·0255	1·026	...	...
At $11^h.20$,	1·004	1·006	1·007	1·011	1·0145	1·017	1·017	1·022	1·024	1·025	...	...
At $12^h.0$	1·001	1·004	...	...	1·013	...	...	...	...	...	...	...

These figures are represented by curves in Plate XIII. fig. 3, and by colours in Plate XIV. fig. 3.

IV*a*. *High Water and Ebb Tide.*—In the backwater near its entrance, where we were driven by the gale, the wind that was blowing *down* the river was blowing *up* (see the arrows in fig. 1, p. 463). There the following observations were made :—

	Surf.	1 ft.	2 ft.	2 ft. 3.	2 ft. 6.	2 ft. 9.	3 ft.	3 ft. 3.	3 ft. 6.	3 ft. 9.	4 ft.	5 ft. 6. bot.
From $12^h.25$ to $12^h.40$,	1·003	1·004	1·005	1·005	1·004	1·004	1·013	...	...	...	...	1·025
	...	...	...	...	...	...	1·005	1·016	1·021	1·023	...	...
At $13^h.25$,	1·000	1·002	1·0025	...	...	...	1·003	...	...	...	1·011	1·014
At $13^h.35$,	...	...	...	...	...	...	...	1·004	1·004	1·005	...	1·005

This shows how the salinity increased until close upon high water, and how it then suddenly vanished. The curves of density are given in Plate XIII. fig. 4, and the diagram of salinity in Plate XIV. fig. 4.]

V. *Ebbing Tide.*—August 7, observations at $12^h.15$; high-water 9^{h}.28. Depth 6 feet. Weather calm, thick mist; surface temperature 58°·0.

The bottom water had a density of 1·014, and on our taking samples at every foot from the surface downwards immediately after this observation, the density was found only to increase from 1·000 to 1·004. The withdrawal of the last of the sea water in a thin layer was observed in this case.

VI. *Ebbing Tide.*—August 8, observations from 10^{h}.40 to 12^{h}.0; high water 10^{h}.59. Depth diminishing from 9 feet to 7 feet. Surface temperature 58°·0.

	Surf.	1 ft.	2 ft.	3 ft.	4 ft.	5 ft.	6 ft.	6 ft. 3.	6 ft. 6.	6 ft. 9.	7 ft.	8 ft.	9 ft.
10^{h}.40 to 11^{h}.20,	1·001	1·001	1·001	1·001	1·003	1·003	1·011	...	...	...	1·013	1·015	1·018
11^{h}.25 to 11^{h}.45,	1·0005	...	...	...	1·001	1·0015	1·002	1·003	1·006	1·007	1·011	1·018	...
11^{h}.45 to 12^{h}.0,*	1·000	...	...	...	1·0015	1·0015	1·004	...	...	...	1·009	...	...

At 12^{h}.0 the density at the bottom was 1·0035.

VII. *Ebbing Tide*—August 21, observations from 10^{h}.30 to 11^{h}.40; high water at 9^{h}.18. River much in its former conditions. Depth, 8 to 7 feet. Surface temperature 55°·8. All the observations made from the bottom upwards.

	Surf.	3 ft. 6.	4 ft.	5 ft.	6 ft.	7 ft.	8 ft.
From 10^{h}.30 to 10^{h}.40 . . .	...	1·001	1·001	...	1·006	1·017	1·023
„ 10^{h}.45 to 10^{h}.50 . . .	...	...	...	...	1·007	1·017	1·023
„ 11^{h}.0 to 11^{h}.5 . . .	...	...	...	1·001	1·017	1·020	...
„ 11^{h}.30 to 11^{h}.40 . . .	...	1·0005	1·0005	1·0005	1·018	...	...

The density observed in this set of experiments is plotted in Plate XIII. fig. 5, and the salinity is shown in Plate XIV. fig. 5.

Tidal Variations of Salinity at the River Mouth.

Taking the observations of No. III. at 16^{h}.55 as typical of the state of matters in the river at half flood, we may represent in the following table (Table I.) the calculated amount of salt present, and the ratio of sea to river water at each point.

Table II. shows the same particulars for a typical case of the ebb tide, that recorded in No. VII., 11^{h}.0.

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* From the bottom upwards.

TABLE I.

Depth in feet.	Density.	Percentage of Salts Present.	Percentage of Fresh Water Present.	Percentage of Sea Water Present.
0	1·001	0·09	99	1
1	1·002	0·45	92	8
2	1·004	0·62	84	16
3	1·014	1·96	44	56
4	1·016	2·22	36	64
5	1·023	3·10	8	92
6	1·023	3·10	8	92
7	1·023	3·10	8	92

TABLE II.

Depth in feet.	Density.	Percentage of Salts Present.	Percentage of Fresh Water Present.	Percentage of Sea Water Present.
0	1·001	0·09	99	1
1	1·001	0·09	99	1
2	1·001	0·09	99	1
3	1·001	0·09	99	1
4	1·001	0·09	99	1
5	1·001	0·09	99	1
6	1·017	2·34	33	67
7	1·020	2·72	20	80

In the coloured diagrams (Plate XIV.) the shades represent roughly the quantity of dissolved salts at each depth as deduced by calculation on the assumptions—which are accurate enough for the purpose of diagrammatic representation—that the indications of the rough hydrometer are correct, and that brackish water is ocean water diluted with pure water.

If in a river running directly into salt water the channel should gradually deepen as it enters the sea, so that there should be no obstacle to the entrance of salt water as the tide rose, and if at the same time it were so narrow as not to be in any sense an inlet of the sea, the conditions in it would be truly comparable with those of a river gradually merging into a firth. This is never the case so far as we have been able to ascertain, as a bar exists at the mouth of all rivers. As a consequence, the sea water enters by first rising to the level of the top of the obstacle, and then pouring down on the other side through the fresh water, and so produces a large volume of brackish or not fully salt water inside the bar.

The action of the breakers over the bar or in the shoal water outside serves still more to effect mixture. Were it not for the great rapidity and volume of the Spey, the tide would probably produce a uniform brackish mixture at the mouth of the river, and a much less marked series of salinity strata farther up, than those that were observed.

The data already given represent the actual manner in which the salt or slightly freshened sea water fights its way up the bottom of the river from the sea, while the fresh river water runs down above. The greatest distance up the stream to which salt water penetrated on the bottom was not satisfactorily determined, but it was certainly less than half a mile. At high tide a very thin layer of salt water was found at the bottom in a deep hole close to the west bank of the river about a quarter of a mile from the mouth. On the east side the shallow backwater (fig. 1) was found on a calm still day to be filled with perceptibly brackish water at two hours' ebb, the fresh upper water having evidently run off most quickly. On another occasion, when there was a gale blowing the fresh surface water into the backwater, the salt layer flowed out from the bottom during the first half-hour of ebb (Plate XIII. fig. 4, and Plate XIV. fig. 4). In this case the pressure of the accumulated water at the upper end of the *cul-de-sac* evidently set up a reaction current outwards, thus reversing the ordinary mode of emptying.

In the open river the sea water appears always to flow and ebb under the fresh water; but some shipmasters, who know the Spey well, told us that at the ebb the river often "ran salt" for a considerable time, the fresh upper water flowing away first; no such phenomenon was presented, according to them, on the flood tide. We looked specially for this effect, but did not observe it.

On one occasion, August 11, two hours before high water, the density from the bottom up to 2 feet from the surface was that of sea water (over 1·024), and the river was brackish even at 6 inches from the surface. A strong wind was driving the river water out to sea on this occasion, and so stripping off the usual layer of fresh water. On another day, August 8, when the ebb was unusually rapid, there was a depth of 6 feet of perfectly fresh water half-an-hour after high tide.

The salinity at any depth during flood or ebb tide is influenced by a number of conditions, such as the density of the sea water coming in, the rate of the out-going current, the height of the tide, the wind, and other variable factors. Supposing, to take the normal conditions, that just outside Spey bar, where the depth is about 1 fathom, the lower $\frac{3}{4}$ fathom has a density of 1·0250 at

60° F., the time being that of low water on a calm day without wind. The river is flowing out by a narrow channel about two feet deep over the bar. The tide gradually rises, and opposes an increasing resistance to the outflowing current, the salt water rises over the bar and creeps up the bottom of the channel, raising the level and so gradually widening the opening by covering sand-banks. At about quarter flood the river begins to rise near the mouth from the damming back of the fresh water, then suddenly (see observations III. on p. 466) a thin wedge of brackish water is found inserting itself between the bottom and the fresh water; a few minutes later there is a layer of salt water a foot deep topped by an equal stratum of brackish, and as time proceeds the salinity at the bottom remains constant, but extends higher, and passes through a brackish zone into the river water, about a foot or eighteen inches of which remains hardly touched by salt, or even quite fresh, and flows out to sea slowly, tearing itself across the layers of denser water flowing at a lower level more slowly but resistlessly upwards. About an hour before full tide there is a state of balance; the wedge of salt water has pushed its brackish edge to the highest point, and the enfeebled force of the tide only suffices to hold back the dammed up water above. This continues for some time; then there is a turn, the salt wedge is forced slowly out, the dammed back water rushes down unchecked, keeping up the level of the river near the mouth for some time, while the salt and finally brackish water withdraws along the bottom; and by half ebb the river down to the bar is entirely fresh, and rapidly returning to its low-water level.

The diagrams on Plate XIV. may be explained on the supposition that water containing less than 0·5 per cent. of salts in solution may be considered *fresh*, while when the percentage of salt is greater, but does not exceed 2 per cent., it is *brackish*, and when more than 2 per cent. of salt is present it is thoroughly *salt*. In order to be truly representative, the shades of colour in these figures should have the margins blended, but reference to the curves (Plate XIII.) will show that if the abrupt transitions did not take place at points corresponding to 0·5 and 2 per cent. of salt respectively, they did take place very near those positions. The diagrams show how with the rising tide the layers of "fresh" and "brackish" water remain of nearly equal depth, while the "salt" water below increases from a mere film on the bottom until it fills more than half of the river bed at the point of observation. The interface between the fresh and salt water is much sharper at some times than at others. We were anxious to find the direction of the plane of contact when it was clearly marked, and to see how it varied at different places, but to make an attempt of this kind a number of observers working simultaneously would be required, and these we could not command.

The theory to which our observations have given rise regarding the mixture of salt and fresh water inside the bar is one which, although not proved, seems to be at least probably true. It is that the fresh water running down the inclined bed of the river meets the wedge of salt water pushing its way up and acting in virtue of its superior density as a sort of soft false bottom sloping in the the opposite direction to the inclination of the river bed. The lower layers of the fresh water have now to run along a level surface or even to force their way up-hill, but in doing so the friction between the two tears off thin streams of salt water, and mixes it with the fresh to form the brackish layer. This is represented diagrammatically in Plate XV. fig. 1. The part that diffusion plays in causing mixture is probably a small one, but it must be increased by the gouging action of the rapid rush of fresh water on the slow push of the salt wedge.*

Observations on Salinity in Spey Bay.

The work outside the river was seriously hampered in consequence of Spey bar being impassable during the least swell, and of the dependent fact that there are no fishing boats at Garmouth or Kingston. Once we went out in a little open boat, and on two occasions in a half-decked yawl from Port Gordon. The rest of the time devoted to outside work was spent on the shore taking samples of water in the surf, westward and eastward, from the river mouth at regular intervals for several miles. The results of this method are made uncertain by the fact—which we in part verified after it was reported to us—that much of the Spey water reaches the sea by filtering through the enormous shingle banks, and so trickling down almost imperceptibly, but still exerting a distinct freshening influence along the shore line.

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* Since writing the above our attention has been directed to a paper by M. Adolphe Guèrard, of Marseilles, read this summer (1885) and printed in vol. lxxxii. of the *Minutes of Proceedings Inst. C.E.*, pp. 305–336, in which he employs (p. 334) almost identical expressions to describe the manner in which a tidal river is affected by the sea. His treatment of the subject is entirely from the engineer's point of view.

VIII. The first trip of about 3 miles straight out from the Spey was made on 30th July, from about 1 hour before high water to three quarters of an hour after it.

Position, . . .		Outside Bar.	1 mile out.	2 miles out.	3 miles out.	¾ mile out.	¼ mile out.	Bar.
Depth, ...		2 fm.	...	6 fm.	10 fm.	...	...	...
Temperature,	Surface,	59·0	58·0	58·0	58·0	58·2	64·0	65·0
	Bottom,	56·5	...	53·7	52·8	...	...	...
Density, . .	Surface,	1·01109	1·02546	1·02497	1·02557	1·02534	1·01339	1·00065
	Bottom,	1·02527	...	1·02587	1·02598	...	...	...

IX*a*. On 10th August, when the river was very low, a trip was made from Port Gordon, and samples were collected on the outward voyage at regular intervals along the circumference of a circle of 3 miles radius, with the mouth of the river as a centre. It was impossible to get the compass placed high enough to enable us to take bearings accurately, so the positions were only approximately determined. The weather was on the whole bright, with occasional showers. There was a stiff breeze from the south-south-west, which became very squally towards afternoon. The five sets of observations made from $9^h.30$ to $12^h.30$, when it was high water, are as follows :—

Position and Depth,		Off Port Gordon. I. 3 fm.	II. 8 fm.	Opposite the Spey. III. 10 fm.	IV. 10 fm.	Near Boar's Head Rock. V. 5 fm.
Temperature,	Surface,	56·6	56·5	56·9	56·8	56·2
	Half depth,	...	56·3	57·6	...	56·1
	Bottom,	56·4	55·3	53·2	53·3	56·0
Density	Surface,	1·02562	1·02554	1·02539	1·02536	1·02576
	Half depth,	...	1·02564	1·02549	1·02531	1·02564
	Bottom,	1·02564	1·02569	1·02584	1·02577	1·02579

On returning nearer land the surface density at Station IV. was 1·02572, and further in-shore it was 1·02376.

IX*b*. When opposite Tugnet (on the east side of the river) the density was observed by the small hydrometer at intervals of a minute while sailing west about a quarter of a mile off-shore until opposite Kingston. Tide 2 hours ebb.

	Off Tugnet, $14^h.18$.			Off the Spey.		Off Kingston, $14^h.25$.	
1·028	1·018	1·014	1·015	1·010	1·010	1·020	1·026.

Off the mouth of Spey in 2 fms. the density at the surface was 1·01051, at 3 feet deep 1·02448, and at the bottom 1·02509. At 15^{h}.15 the density at Station II. was 1·02551 at the surface and 1·02557 at the bottom in 6 fathoms.

X*a*. Another trip from Port Gordon was made on 18th August, between 10^{h}.50 and 16^{h}.40. The river, which had come down in heavy spate on the 13th, was still unusually full. The weather was bright and warm, the wind very light, and a considerable swell was coming in from the north-east. The wind was dead on land, and too light for the boat to sail up to, consequently we had to keep near the shore, about half a mile out, while going westward, until off the Spey. The water was swarming with medusæ of all sizes and colours. The following observations were made in the course of the day, the densities being determined on shore by the delicate hydrometer, and reduced by calculation to 15°·56 C.:—

Position.		Off Buckie.	Off Port Gordon.	Halfway to Spey.	Off Spey Bar.		Between Spey and Port Gordon.	
					½ mile out.	¼ mile out.		
Hour and tide, . .		10^{h}.50. 5 hrs. ebb.	11^{h}.45. l.w.	12^{h}42. ½ hr. fld.	13^{h}.50. 1¾ hr. fld.	14^{h}.15. 2 hrs. fld.	15^{h}.15. 3 hrs. fld.	15^{h}.55. 3¾ hrs. fld.
Depth, ...		8 fm.	6½ fm.	5 fm.	4½ fm.	4 fm.	7 fm.	5 fm.
Temperature,	Surface, .	54·9	55·1	56·2	57·1	56·1	57·7	57·1
	Half-depth,	55·2	55·2	55·9	55·6	...	55·6	55·7
	Bottom .	53·8	55·2	55·3	55·2	...	55·0	55·3
Density,	Surface, .	1·02285	1·02383	1·02445	1·01227	1·02435	1·02267	1·02328
	Half-depth,	1·02548	1·02554	1·02403	1·02535	1·02525	1·02555	1·02538
	Bottom .	1·02318	1·02562	1·02544	1·02536	1·02545	1·02554	1·02540

Quarter of a mile off Port Gordon the surface density was 1·02164, and at the harbour mouth 1·02140.

X*b*. When we were in the neighbourhood of the mouth of the river, a great many observations were made with the small hydrometer. The high density of the sample of water taken home from near the river mouth (1·02435 at the surface) must be explained by the boat's drifting a little to the east of the fresh current before the sample was bottled, for while coming straight in steering for the bar from the previous position, the readings of the small hydrometer were—

1·013, 1·015, 1·012, 1·0125, 1·0125, 1·010, 1·0085, 1·0085.

Returning from that position, and steering N. by W., in the direction of the axis of the river, we found the density to vary as follows:—

1·026, 1·022, 1·020, 1·018, 1·0185, 1·019, 1·0195,
1·020, 1·021, 1·023, 1·0235, 1·0245, 1·0245, 1·025.

The temperature steadily rose from 57·3 until at the point where the density was 1·021 it was 57°·8; it then fell gradually to 57°·1 at the point where the density was 1·025, about $1\frac{1}{2}$ mile off-shore.

X*c*. From that position we steered SE. by E. straight for Port Gordon until about 1 mile from the harbour. The temperature rose steadily from 57·1 to 58·2, and the density varied as follows:—

1·026, 1·0265, 1·026, 1·0255, 1·025, 1·025, 1·0245,
1·024, 1·024, 1·023, 1·023, 1·022, 1·022, 1·021,
1·020, 1·0205, 1·0215, 1·022, 1·025, 1·024.

The approximate distribution of salinity is shown in Pl. XV. fig. 2.

XI. On August 26, at $8^{h}.15$, by the kindness of Captain Linklater of the "St Clair," a sample of surface water was taken in the centre of the Moray Firth between Aberdeen and Wick. The weather was bright, with an easterly swell. The temperature of the water was 52°·7, and the density, afterwards determined, as 1·02612. Another sample was taken on September 18, at $10^{h}.30$ when off Stonehaven, about 2 miles from shore; the weather was bright and clear, the sea calm, air temperature 55°·4, and that of surface water 54°·5. The density was found to be 1·02592.

Salinity of the Sea Margin.

Observations were made on several occasions along the shore at regular intervals. The density was observed by means of the small hydrometer, the samples of water being collected in the surf by wading as far out as circumstances would permit.

XII*a*. On August 15 the river, which had been in high flood on the 13th and 14th, had subsided considerably, but was still flooded. A swell coming in, caused a heavy surf. Samples were taken every $\frac{1}{4}$ mile from a position $1\frac{1}{4}$ mile east of the river mouth, westwards to the river; time, from $10^{h}.45$ to $12^{h}.20$. High water, $16^{h}.27$.

1·026(58°·0), —(58°·0), —(56°·5), 1·021(56°·2), 1·021(56°·0), —(56°·3).

XII*b*. August 16. From $15^{h}.30$ to $16^{h}.25$ (high water) the following observations were made at the same places:—

1·026(56°·9), 1·0245(57°·2), 1·024(57°·6), 1·021(58°·0), 1·0245(57°·2), —(—).

XIII*a*. August 17. From Port Gordon, $2\frac{1}{2}$ miles east, to mouth of Spey. High water, $17^{h}.20$. Observations every $\frac{1}{4}$ mile.

From $1\frac{1}{2}$ miles east of Spey to Port Gordon, $11^{h}.35$ to $12^{h}.20$.

1·020(56°·7), 1·023(56° 2), 1·0225(57°·0), 1·026(59°·3).

1½ mile east of Spey to river mouth, 13^h.10 to 14^h.25.

—(58°·0), 1·020(57°·8), 1·0215(57°·3), 1·022(57°·0), 1·023(56°·8), 1·023(56°·5) —(57°·02).

From 1¼ mile east to 1¾ mile east. 16^h.50 to 17^h.10.

1·0225(56°·3), 1·0215(56°·5), 1·022(56°·6).

From 1 mile east westward to river mouth, 17^h.25 to 18^h.0.

1·024(56°·2), 1·025(56°·1), 1·024(56°·0), 1·024(56°·0), 1·018(55°·8), 1·014(56°·1).

XIII*b*. Near the mouth of the river on the west side density at 20^h.0 was 1·014, and ¼ mile west from it 1·027.

On 19th August a large number of observations was made on the shore to the west and to the east of the river:—

XIV*a*. High water at 7^h.0. On the west shore observations were made on the sand-bank at the mouth of the river, which was just uncovered at 10^h.2. On the south (riverward) side of it the temperature of the water was 56°·3, and the density 1·004; on the north or seaward side the temperature was 55°·6, and density 1·023. Passing up the river we found the water to become perfectly fresh within 200 yards of the bar.

Temperature and density were observed along the shore for about 1¾ mile to the west, both going and returning at the same points. This occupied from 10^h.30 to 11^h.42 in going, and from 12^h.10 to 12^h.55 coming back. Observations from the river mouth made every 200 yards westward:—

	1	2	3	4	5
Going, .	1·0225(55°·7)	1·022(56°·0)	1·021(55°·7)	1·023(56°·0)	1:023(56°·0)
Returning,	1·022(56°·3)	...	1·0225(57°·0)	1·023(56°·4)	1·023(56°·3)

Then at distances of 400 yards—

	6	7	8	9	10
Going, .	1·022(56°·0)	1·023(56°·0)	1·0235(56°·0)	1·023(55°·9)	1·023(55°·9)
Returning,	...	1·0235(56°·3)	1·0225(56°·0)	1·023(56°·0)	1·024(56°·3).

XIV*b*. On the east side observations were made eastward from the river mouth for about 2 miles, the time occupied being from 14^h.15 to 16^h.16. High water at 19^h.40.

At intervals of 100 yards from the river mouth—

1·002(58°·5), 1·004(58°·2), 1·005(59°·3), 1·006(59°·3), 1·004(58°·5), 1·015(57°·3), 1·014(57°·2), 1·015(57°·0), 1·017(56°·7).

Then at points 200 yards apart—

1·020(56°·8), 1·021(56°·8),

and at intervals of 400 yards—

1·0205(57°·0), 1·021(57°·3), 1·020(57°·6), 1·020(57°·8), 1·021(58°·0), 1·019(58°·7).

XV. On 20th August observations made at Portessie, 6 miles east from the Spey, showed at $9^h.50$ a density of 1·026, at $11^h.5$ of 1·026, and at $18^h.15$ of 1·027.

The Salinity of Spey Bay.

The mean density at 15°·56 C. of the bottom water in Spey Bay was found to be 1·02567. It varied in twelve cases from 1·02540 to 1·02598, while the surface water varied in density from 1·02000 to 1·02576. The observations made seem to warrant the conclusion that the water is, as a whole, of a density slightly greater than 1·02550 in Spey Bay, and considerably greater in the centre of the Moray Firth. Whether the slight reduction in density is due to the action of the Spey water, or to the vicinity of the Cromarty and Inverness Firths, can hardly be said, and it is unimportant in connection with the immediate subject of which we treat.

The Spey water runs rapidly out of its comparatively shallow channel: at low tide, partly on account of its shallowness, and at high tide, partly on account of the bar, it flows across the surface of the sea water in a thin stream, freshening to a marked extent only the upper few feet, and spreading out laterally in the form of a fan. The exact shape and direction of this expansion are dependent on the volume of the river, on the state of the tide, on the conformation of the mouth, which is subject to incessant change, and on the direction of the wind. The usual trend of the river current in the bay appears to be north-easterly.

The observations VIII. (p. 472) show that when the boat sailed due north, the density at ¾ mile from the river mouth was 1·02557 on the surface, strong sea water; while those under IX*a*., with the river in the same state as regards flood, show no clear indication of the course of the stream on the circumference of a circle 3 miles in radius described from the mouth of the Spey. The pocket hydrometer readings, IX*b*., show that when ¼ mile off shore the stream was clearly defined, its margin shading off gradually on the eastern side and abruptly on the western, where the transition from brackish to salt water was very sharp. The stiff breeze which was blowing from the S.S.W. would help to carry the stream to the N.N.E., and it might have been narrow enough to pass between two of the observing points and so elude detection.

The observations X*a*. show an entirely different set of conditions. The river was partially flooded, and the wind had been easterly for some days, thereby probably keeping the freshened surface water from spreading to the eastward. The result was that, within a mile of the shore there was an area, the surface density of which varied from 1·012 to 1·024, the bottom water being scarcely below the normal (about 1·0256). The observations, X*b*. and X*c*., with the small hydrometer, show how the density increased steadily in a northerly direction from the river until the water was quite salt on the surface, and how, when the boat was turned towards Port Gordon, the salinity at first increased and then diminished as the fresh current sweeping to the eastward against the flood tide was entered on. The diagram, Plate XV. fig. 2, gives an idea of the manner in which these, combined with numerous other observations not recorded in detail, indicate the direction in which the river runs through the bay. The colouring shows the difference between the salt and brackish surface water, the former being represented by the darker shade. Density by the small hydrometer is marked at intervals along the course of the boat.

It is evident, from the numerous observations of density made along the beach by means of the small hydrometer, that the shore water is much fresher on the east than on the west side of the river, and that on going eastward from the river the observer finds the density increase to a certain point (XIII*a*.), and then diminish, as if the current at first swept out to sea, and then curved back along the shore. This is indicated in a general manner in the diagram referred to above. The series XIV*a*. and XIV*b*. show the greater freshness on the east side very clearly, although on the day when those observations were made the salinity on the west side was considerably less than usual. The generally-accepted fact, which we in part verified, that much of the Spey water finds its way to the sea by percolating through the shingle, may account for the uniformly low density along the shore.

The phenomenon of percolation is presented on the small scale by the Tynet burn, near Port Gordon (Plate XV. fig. 2). It is a small stream flowing into a long, narrow deep pool without an outlet, one side of which is a shingle bank, sloping down to the sand. Its length is parallel to the shore line, and nearly at right angles to the course of the burn. Although there is no apparent outlet, close examination shows that an immense number of trickling streamlets, varying in breadth from half an inch to about a foot, and in depth from that of a mere film to several inches, escape all along the lower margin of the bank, and meander across the sand to the sea, the edge of which is perceptibly freshened by this means. The condition of outflow is represented in the diagram, Plate XV. fig. 4, fresh water by a red, sea water by a blue colour.

The variation of salinity with depth throughout Spey Bay depends almost entirely on the amount of flood in the river, and on the direction in which it runs. On August 10, when the river was low, and the boat in cruising about the bay did not encounter any freshened surface water, there was, as a rule, a uniform and slight increase of density with depth; while on August 18, when the river was flooded and the surface water of a great part of the bay comparatively fresh, the bottom and half-depth densities remained as before, and the surface was greatly freshened, although to a slight depth. One or two cases were observed where the water from half the depth was denser than that from the bottom, and one in which it was less dense than at the surface. These differences may be due to irregular and incomplete mixing of river and sea water.

The course of the fresh water stream through the bay is beautifully seen during a flood. Fishermen told us that when a heavy spate occurred with a southerly gale they have seen the coloured water running due north in a wide stream, but that it usually runs towards the east.

A great spate occurred on the night of August 12, the largest, according to the ferryman at Garmouth, for over fifteen years. The average state of the water during our stay had been lower than it had almost ever been observed before. On the morning of the 12th the river was low, and the water very clear and perfectly colourless. At 17^h.30 the water had a clear amber colour, was very slightly increased in volume, and had the temperature 50°·0. Next morning (August 13) the river was a broad sheet of leaping yellow waves, sweeping from bank to bank, and carrying down planks, salmon boxes, and potatoes, a field of which had been flooded and destroyed some miles up the river. As the current crashed round the shingle heaps with tremendous velocity great slices of the bank, from a few inches to more than a foot thick, 8 to 10 feet high, and many yards long, were undermined, and rushed down into the stream with a hoarse rattle. From the rate at which these banks were carried away it could be easily understood how the sea in the neighbourhood of the mouth is constantly shoaling. In the forenoon, with the tide at half-flood, and a gale blowing from the north-west, the whole bay was bordered by a broad band of surf; the waves first breaking more than a quarter of a mile from shore, re-formed in the foam to break and form again over and over before the final dash upon the shingle. As far as vision extended the sea, reflecting the intense blue of the sky, was tasselled with feathery jets of foam, shooting up with constant change of form and position all over its surface; and across it, as if drawn by a brush dipped in brown paint, lay the turbid stream of the river, disappearing towards the north-east. It was seen in the afternoon quite distinguishable from the rest of the water at Portessie, 6 miles away, where it hugged the shore, but not so closely as to prevent a belt of clear water lying within it. Where the line of breakers crossed the river mouth, the white colour of the waves gave place to yellow. As the river shot out the clear ripples of the sea waves ran up from the north-west, struck the rapid-running stream of muddy fresh water, and seemed to recoil as if from a solid obstacle: the whole stream was bent abruptly towards the east, no discoloration of the water being perceptible to the west of the river mouth. The diagram (Plate XV. fig. 3), where the sea water is shown in blue and the river water in brown, shows the manner in which the stream curved, and hardly exaggerates the contrast of colour it presented. The temperature of the river near the mouth was 48°·3, a fall of 6°·9 from the previous afternoon; that of the surf at the river mouth was 51·5, and a quarter of a mile west it was 55°·0. It was matter of extreme regret that no observation could be make on the bay or on the east side of the river, but no boat could face the weather. The roar of wind and thunder of surf, the rattle of the pebbles along the shore, and the periodical crash of the bank into the stream, were sometimes almost drowned by the sound of an impetuous hail-shower, which one moment was tearing up the sand and gravel, and the next sweeping over the country, followed by magnificent sunshine.

Alkalinity.

We intended to observe the alkalinity of numerous samples of sea and river water, and for this purpose we had procured the necessary standard solutions from Professor Dittmar. Practical difficulties prevented the carrying out of this plan, and few determinations were made. It is advisable to mention the results obtained (Table III.), meagre though they be. The only conclusions we can draw from the consideration of them, and these we present with some diffidence, are—

1. The alkalinity (which is a measure of the potential carbonate of lime present) is less, for waters of the same density, than in the Firth of Forth.

2. In water taken on different occasions from the same position the alkalinity is *not* proportional to the density, but appears to be liable to variation.

3. There are indications that the alkalinity of the water in Spey Bay is, for the same density, greater to the east of the river mouth—that is, in the direction of the fresh current. This may be due to the carbonate of lime carried down in solution by the fresh water.

4. There are no distinct indications of a uniform change of alkalinity with depth, although surface water is more alkaline than that at the bottom, even when the density of the former is less.

In Table III. *alkalinity* is represented by the number of milligrammes of carbonic acid (CO_2) existing in normal carbonates ($R''CO_3$) in one litre of water. Thus, if a sample had alkalinity 50·00, it contained 50 milligrammes of carbonic acid in combination with lime or with some other base, equivalent to 113·7 milligrammes of normal calcium carbonate ($CaCO_3$).

Temperature of the Water.

The temperature of the water in the bay was lower than in the river. The water at the bottom was colder than at the surface, the difference being about 3°, where the depth was 10 fathoms. Full particulars of the observations are given under VIII., IX*a*., X., XI., and of those along the shore under XII., XIII., XIV.

The temperature of the river was observed once or twice daily

TABLE III.—*Alkalinity of Water in Spey Bay.*

Date.	Position.	Depth.	Position of Sample.	Density at 15°·56.	Alkalinity.			
					I.	II.	III.	Mean.
1.8.85	Inside Spey Bar, ...	1½ fm.	S.	1·00108	13·04	13·40	...	13·22
17.8.85	Shore at Port Gordon, . . .	...	S.	1·02368	46·84	...	...	...
30.7.85	1 mile off Spey mouth, . . .	...	S.	1·02546	47·40	48·16	49·80	48·45
10.8.85	West of Kingston, near shore, . .	...	S.	1·02576	48·48	48·52	...	48·50
10.8.85	Spey Bay, ...	8 fm.	4 fm.	1·02564	49·44	...	...	...
30.7.85	3 miles off mouth of Spey, . .	10 fm.	B.	1·02598	50·40	50·40	50·20	50·33
10.8.85	3 miles off shore, E. of river, . .	„	5 fm.	1·02531	50·48	50·24	...	50·36
10.8.85	„ „ . .	„	B.	1·02577	51·24	51·00	...	51·12
10.8.85	„ „ . .	„	S.	1·02536	52·04	52·00	...	52·02

TABLE IV.—*Temperature of River Water.*

Date, .	July		August														
	30	31	1	3	5	6	7	8	11	12	13	14	15	17	19	21	24
Forenoon observation,	62·3	62·0	62·3	59·0	56·2	57·3	57·3	58·0	57·2	...	48·3	...	53·0	55·2	...	55·8	56·7
Afternoon observation,	66·0	66·5	63·0	60·0	57·2	...	58·0	...	...	55·0	...	52·8	57·0	...	58·9	...	...
Range, . .	3·7	4·5	0·7	1·0	1·0	...	0·7	...	...	...	...	...	4·0	...	...	...	...

when it was possible for this to be done. Table IV. shows the result of the observations, which are unfortunately very fragmentary. The rise of temperature between the hours 10 and 18 was found to be about 2° as an average. For the month the variation may be described as a gradual fall of temperature with a sudden depression on the 13th—the day of heavy spate already alluded to—from which there was a partial recovery.

CONCLUSION.

The importance to engineers of a knowledge of the relationship between salt and fresh water at the mouths of rivers is generally allowed; and much information regarding the variations of salinity in estuaries with tide and depth doubtless exists in the note-books of marine surveyors, but so far as we know, no definite investigation of the matter has been published. Since our work was done without knowing the volume and velocity of the fresh water stream, there is a want of that quantitative element which gives precision to an engineering survey. The two objects are, in fact, distinct; but so much can be done by each, that it would form a definite advance in our knowledge of the circulation of water in the world if a conjoint study of the physical properties of a series of typical rivers and firths could be made simultaneously by a chemist and engineer, somewhat on the lines of Mr R. W. Peregrine Birch's work on the Thames,* but in a more complete and satisfactory manner.

A quantitative analysis of the soluble mineral constituents in the Spey river water is at present being carried out by one of us, with the view of ascertaining the influence of the river water on the ratio to each other of the principal components of the salt in the sea water near the estuary.

Our work on the Spey corroborates the observations of engineers on the water at the mouth of tidal rivers, and shows more precisely than has to our knowledge previously been done, in this country at least, how sea and river water meet and mingle. We are also able to divide river estuaries into at least three classes, the conditions of all these being perfectly distinct.

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* *Min. Proc. Inst. C.E.*, lxxviii. 212, and lxxxi. 295.

1. *Rivers without Firths,* such as the Spey.—In such rivers the water inside the bar is entirely fresh during the greater part of ebb-tide, since all the sea water which forces its way up the short estuary during flood-tide runs out again along the bottom, and does not at any time render the surface layers brackish. The fresh water, running out at all states of the tide, spreads over the surface of the sea, and persists in a thin layer or in patches to a considerable distance, overlying water of uniformly high salinity. The conditions as to temperature are not fully known, but except during certain periods in spring and autumn, the river water is either much warmer or much colder than that of the sea.

2. *Rivers with narrow, shallow Firths,* such as the Tay.—The bar lies out at sea, where the water is always salt. The tide exerts a great influence throughout the estuary, the sea water being only partly withdrawn by the ebb; and the currents, setting up and down with great velocity, effect a tumultuous though transitory mixing of the water. During ebb-tide the water inside the bar is comparatively fresh at the surface, and the ridge it has to pass over in reaching the sea directs it out over the surface of the salt water in a brackish layer, like a river of the first class. The temperature relations are not well known, but appear to be similar to those of estuaries of the third class. The range is, however, greater, and variations more irregular.

3. *Rivers with wide and deep Firths,* such as the Forth.—Here the sea water is never withdrawn from the estuary by the tide, and the tidal currents are less rapid than in rivers of the second class. The landward portion (the "river proper compartment" of engineers) is comparable to a river of the first class, but the conditions are much less distinctly defined. Further seaward comes a division (the "tidal compartment") resembling an estuary of the second class, without a bar, but exhibiting all its other characteristics in a modified degree. Finally, nearest the sea, and comprising most of the firth, there is the region specially characteristic of this kind of river entrance. The difference of density between surface and bottom is slight, and it is scarcely affected by the tides; the water throughout its whole mass is slightly freshened, and so meets the true sea water beyond as a great wall of only slightly reduced salinity. The curve representing the salinity of the surface water is

very nearly constant; floods affect it slightly, but uniformly. It shows that in the "river proper" and "tidal" compartments there is rapid increase of density per mile of length, and much influence by tide; while, as the firth is entered on, the increase of density per mile becomes less and less, and the tidal disturbance vanishes.

The temperature rises steadily from the river out to sea in winter, and the bottom water is warmer than that at the surface; in summer the temperature rises from the sea towards the river, and the surface water is warmer than that below. The annual range of temperature in the water is greater the further up the river observations are made, but the mean temperature appears to be nearly the same everywhere.

SUMMARY.

The Spey, the most rapid river in Britain, is 120 miles long, and drains 1245 square miles. It flows into the Moray Firth—a bight of the North Sea—at Garmouth; the river entrance is shallow, and subject to change of form and position by shifting banks.

Observations on the effect of tide on the mixing of sea and river water inside the bar are described. The exact vertical distribution of salinity was investigated by means of the hydrometer at intervals during the rise and fall of the tide, and the results are embodied in tables, diagrams, and curves. They show that the sea water slowly forces its way like a wedge between the river water and the bottom as the tide rises, and dams back the water further up stream; while the surface water always remains quite fresh, and a brackish zone separates the two strata. When the ebb sets in the salt water runs out very rapidily, and before half-ebb there is only fresh water inside the bar.

The salinity of the water in Spey Bay was studied during short trips in fishing-boats from Garmouth and Port Gordon, and by observations made along the shore. These are fully recorded. The bottom water of the bay was found to be of the density 1·02567 on an average; the surface water varied much in density; and the river's course could be traced as a stream sweeping to the north-east. The western margin of this stream was sharply defined; on the east no abrupt change can be found, the river water gradually merging into the sea. During a spate the discoloration of the river water enables its progress through the Firth to be traced as a dark band to a distance of more than 6 miles. The shore water appears to be freshened by water percolating through the shingle beaches from the river.

Alkalinity and temperature observations are described and discussed.

In conclusion, a classification of river entrances into three groups is put forward tentatively. The Spey typifies a large river entering a tidal sea directly across a bar; the Tay, a large river, entering by a short, narrow, shallow, barred estuary; and the Forth, a small river, merging into a long, wide, deep, and open firth.

7. The Distribution and Significance of Micro-Organisms in Water. By A. Wynter Blyth.

The results of more than a year's study of the distribution of micro-organisms, that is, bacteria and fungi in water, are I think of sufficient interest to justify a short communication.

Methods.—The micro-organisms in water being for the most part in the form of isolated spores or seeds so small as to be practically invisible, the first step was to make them visible by "cultivation." This cultivation took place in nutrient gelatin, according to principles too well known to require description. I will, therefore, confine my remarks merely to the details of certain special modifications of the ordinary process which I adopt.

The cultivation takes place on glass plates 4 inches × 4 inches; on to these plates are cemented glass rings $\frac{1}{4}$ inch broad, $\frac{1}{4}$ inch deep, and 3·8 inches in diameter, thus forming large shallow cells. The method of cementing the rings on to the plates is novel; both are sterilised at a high temperature in a hot-air oven, and while still hot drops of the nutrient gelatin are placed on the ground surface of the ring, the ring is then applied to the plate, rotated, and put under a dust-proof shade to cool and set.

The water to be examined is weighed in a special form of drop-bottle (see fig. 1). A drop is then dropped on the plate by the aid of the pipette stopper, and the bottle reweighed so as to obtain by difference the weight of the drop. The nutrient gelatin is melted at a gentle heat in a Lister flask, and sufficient poured into the cell so as to mix with the drop of water and to form a layer of one-tenth of an inch deep. The glass cell thus prepared is placed in a special glass shade or chamber, the air of which is kept saturated with moisture. After from three to five days, the colonies developed are counted by the aid of a lens. To facilitate the enumeration, the plates are all ruled by a writing diamond into squares.

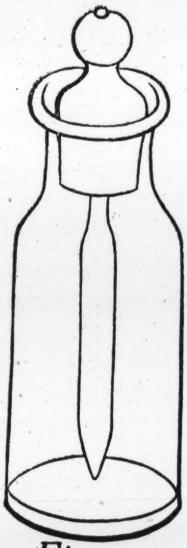

Fig. 1

The collection of ordinary samples was by means of small 2-oz. phials, washed out with boiling water, and then heated for many hours in an oven to about 250° C.

In order to obtain samples of water at different depths, a very efficient and simple apparatus was invented for me by an ingenious friend.* To the disc of wood *a* (see fig. 2), weighted with lead, is fixed a frame of brass *g*; in each side of the frame are slots, in which a clip *x* slides freely. A small stoppered bottle is firmly attached to the disc, and the clip secured to the stopper; the clip, and with it the stopper, is firmly held down by means of two pieces of elastic at *s* and *s′*; the apparatus is let down to any required depth by means of a line attached to the stout copper wire *w*; to take deep sea samples the line is best marked into fathoms like a deep sea line. The stopper is opened at any time by pulling on a string attached to the ring *r*; on releasing this string the stopper is immediately pulled into its place by the elastics.

Sewage.—I found it practically impossible to make a quantitative determination of the micro-organisms in sewage unless the liquid was very much diluted. A convenient dilution is 1 c.c. of sewage to a litre of sterilised water. Drops of this dilute sewage, weighing from 40 to 50 milligrams, and added to nutrient gelatin, yield, after a few days' cultivation, from 100 to 200 colonies—numbers the counting of which present no difficulty, and a simple calculation gives the amount per gramme of the original sewage. The following are examples of results obtained from London sewage :—

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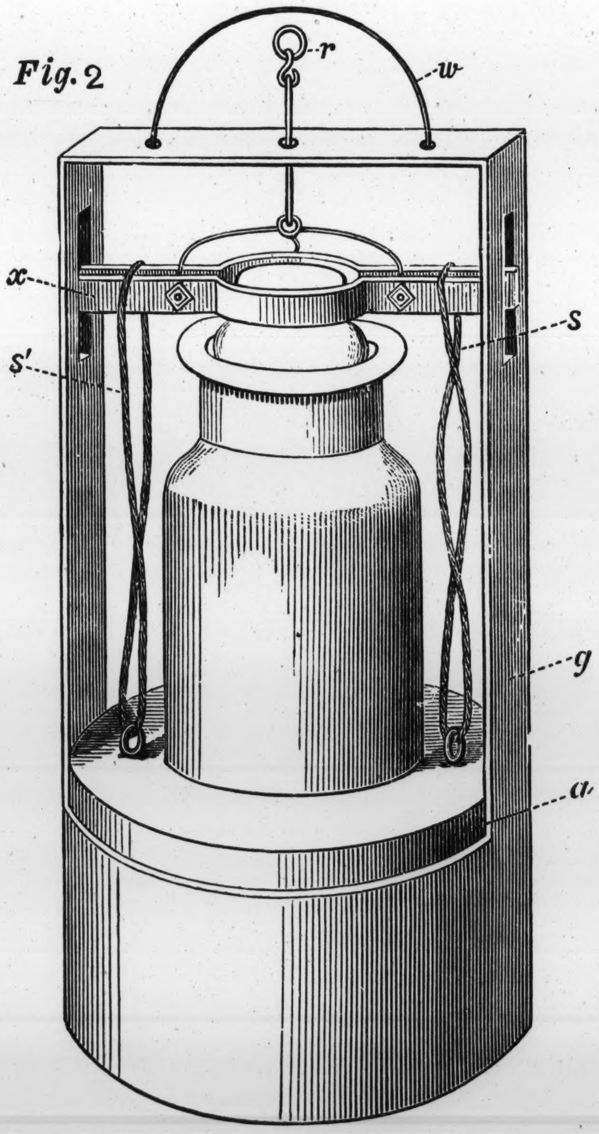
* Mr Stafford L. Archer, whom I have to thank for the trouble he took in perfecting the mechanism.

	Colonies per grm.
London sewage, June 1885, ...	1,490,000
London sewage, July 1885, ...	4,226,804
London sewage, November 1885, ...	773,800

It is therefore evident that the sewage is considerably influenced by season, containing more in the summer than in the winter season, but for the most always yielding colonies approaching to or

Fig. 2



above a million per gramme. The colonies are of all kinds of fungi, more especially aspergilli, bacteria, bacilli, and micrococci.

Pond Water and open Wells.—A typical open pond which received no drainage, and was frequented by ducks and geese, contained water which yielded to cultivation 3534 colonies to the gramme; 465 of these were bacilli liquefying the gelatin, and 47 were bacteria, agreeing in general characters with *Bacterium termo.*

An open well in Devonshire gave the following analytical values:—* Nitrogen as nitrates, ·0984; chlorine, 3·2; ammonia, 0014; albuminoid ammonia, ·0078; oxygen consumed in 15 minutes, ·0648; oxygen consumed in 4 hours, ·1666; hardness, 22·9; total solids, 32·5. The ordinary microscopical examination detected epithelial cells, vegetable débris, and moving animalcules; 35 mgrs. of the water yielded to cultivation 330 centres, 4 of which slowly liquefied the gelatin. This is equal to 9428 per gramme.

Canal Water.—The Regent's Canal, in its course through the Metropolis, has been for years little better than a stagnant ditch, and the composition of its waters approximates to that of dilute sewage. A sample, taken a foot below the surface, gave colonies equal to 32,352 per gramme; whilst, just above the mud, another sample gave 521,739 per gramme.

River Water.—The water of a stream with an appreciable velocity shows the same general fact, viz., more micro-organisms near the bottom than the surface; *e.g.*, samples taken from the Thames at Sunbury just opposite the intake of the water companies, yielded to cultivation the following figures:—

Near the surface, ...	11,050 per grm.
Close to the bottom, ...	59,800 ,,

In the same locality a second sample, taken above some black mud, yielded colonies equal in number to 6,430,000 per gramme.

It thus appears that in canals and rivers the bacteria slowly subside, probably by adhering to the mineral and organic substances which are continually sinking. That the richness of the bottom layer in bacteria is intimately connected with the precipitation of particles, is evident from the following experiment:—A wide upright glass vessel 2 feet 5 inches in height was filled with sterilised water, 1 c.c. of sewage added, the whole shaken, and then allowed to stand for twenty-four hours; at the end of which time samples were taken from the surface, the middle and bottom respectively, with the following result:—

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* In grains per gallon.

	Colonies per grm.
Surface,	411
Middle,	333
Bottom,	160

Sea Water.—In the sea water around the English coast, the water at or near the surface seems to be uniformly richer than at deeper depths.

A sample of sea water, taken off the old Pier at Brighton in July, gave the following results :—Surface, 50 mgrms. yielded 843 centres—that is, 16,860 per gramme; while a sample from the bottom only yielded 230 per gramme.

A sample of sea water, taken from the surface of the Yarmouth coast in September, gave 14,887 per gramme. Dr Vacher of Birkenhead kindly supplied me with samples of sea water, taken from the surface and at the depth of 20 fathoms off Holyhead, and the number of colonies at the surface gave a very similar result while the deep sea sample yielded comparatively few centres.*

Cultivation of ordinary Drinking Water.—For some time past I have made cultivations of all waters analysed by me in my capacity of a public analyst, and have now sufficient experience to justify a judgment as to the utility and limits of the method. A public water supply, derived from flowing rivers, or from any surface water, varies as to its composition according to season, the more especially as the water has a winter character due chiefly to winter rains, and summer character due chiefly to the summer drought. It hence follows that a standard of purity, which does not take into account natural seasonal variations, cannot be correct.

The water supplied to London by the West Middlesex and Grand Junction Companies is analysed by me month by month, and since April have also been submitted to cultivation. The analytical results, and the number of organisms per gramme, are detailed in

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* I have to my great regret entirely lost my notes of the numbers, but as stated, the general facts agreed with the experiments on the shallower waters nearer the coast.

Table I. It may be useful here to allude to a similar investigation published in the *Arbeiten aus dem Kaiserlichen Gesundheitsamte*, Band I. Berlin, 1885, on the water supplied to Berlin. The cultivations were made daily from July 1884 to March 1885. The mean of the numbers of colonies raised from unfiltered river water (Stralauer Works) was 1435 per gramme, the maximun being in October 3251, the minimum in February 685. Filtration altered the water very considerably, the mean of the filtered water being 107 (maximun 277, and minimun 21).

Lake water (Tegeler Works) when unfiltered gave a mean for the nine months of 441, the maximum 890 falling in March, the minimum in August. The mean number of colonies in the filtered lake water was 51, and the extremes were 14 and 121.

It is therefore clear that the filtered Berlin river water is, so far as bacterial life goes, purer than the filtered London river water, the mean number of colonies per gramme being for the West Middlesex Company 468, and that of the Grand Junction 261.*

With regard to other waters, I give in the following tables examples of good, indifferent, and polluted waters, from which I think it may be concluded that, irrespective of the special kind of micro-organism in a water, any number of colonies which exceed 1000 per gramme is indicative of considerable impurity.

Summary.—To briefly sum up the facts already detailed—

(1) Impure liquids, such as sewage, pond and canal waters contain hundreds of thousands or even millions of micro-organisms per gramme.

(2) In canals and rivers the number of micro-organisms at or near the bottom, is greater than at or near the surface.

(3) In sea water the conditions are reversed, the number of organisms being greater at or near the surface.

(4) A good water should not contain more than 1000 colonies of micro-organisms in every gramme.†

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* My numbers for the London waters have been hitherto in excess of the one or two other observers working at this subject. I always cultivate the water twenty-four hours after collection, during which period of rest there may be some considerable increase, especially in summer.

† That is, when cultivated within twenty-four hours after collection.

TABLE I.

Monthly Analyses of the Water supplied by the Grand Junction Water Company, with the Number of Colonies of Micro-Organisms per gramme.

	May	June	July	Aug.	Sept.	Oct.	Nov.
	Grains per gal.	Grains per gal.	Grains per gal.	Grains per gal.	Grains per gal.	Grains per gal.	Grains per gal.
Chlorine, ...	1·3	1·15	1·30	1·25	1·20	1·20	1·3
Nitrogen as ammonia, . .	·1398	·1362	·1309	·1453	·1403	·1307	·1601
Free ammonia, . . .	·0009	·0006	·0006	·0008	·0007	·0010	·0009
Albuminoid ammonia, . .	·0039	·0036	·0048	·0063	·0051	·0061	·0039
Oxygen consumed in 15 minutes at 100° F., . .	·0714	·0388	·0431	·0592	·0292	·0386	·0047
Oxygen consumed in 4 hours at 100° F., . . .	·1050	·0967	·0754	·1001	·0544	·0648	·1680
Hardness before boiling, .	11·3	11·79	12·59	12·49	12·00	14·91	13·91
„ after boiling, .	2·9	2·85	3·44	1·76	2·44	2·64	2·23
Total solid residue, . .	17·0	17·0	18·0	17·5	18·0	20·5	20·0
No. of colonies of micro-organisms in 1 gramme of the water, . . .	160	68	276	1000	96	173	54

TABLE II.

Monthly Analyses of the Water supplied by the West Middlesex Water Company, with the Number of Colonies of Micro-Organisms per gramme.

	April	May	June	July	Aug.	Sept.	Oct.	Nov.
	Grains per gal.	Grains per gal.	Grains per gal.	Grains per gal.	Grains per gal.	Grains per gal.	Grains per gal.	Grains per gal.
Chlorine, ...	1·25	1·20	1·20	1·25	1·15	1·20	1·2	1·2
Nitrogen as ammonia, . .	·1438	·1264	·1438	·1273	·1320	·1487	·1260	·1570
Free ammonia, . . .	·0012	·0014	·0004	·0008	·0006	·0008	·0008	·0007
Albuminoid ammonia, . .	·0084	·0045	·0041	·0052	·0051	·0057	·0042	·0039
Oxygen consumed in 15 minutes at 100° F., .	·0749	·0636	·0466	·0538	·0329	·0233	·0518	·0020
Oxygen consumed in 4 hours at 100° F., . .	·1631	·1225	·1414	·0861	·0686	·0472	·0777	·1540
Hardness before boiling, .	12·63	10·8	12·93	11·38	11·84	12·66	13·76	12·78
„ after boiling, .	2·48	3·1	2·64	3·74	1·91	2·85	2·38	2·05
Total solid residue, . .	17·00	16·5	18·0	17·5	16·5	18·5	19·0	18·0
No. of colonies of micro-organisms in 1 gramme of the water, . . .	90	140	225	1223	125	979	583	30

TABLE III.

Examples of Good Waters.

	Chlorine.	Phosphoric Acid.	Nitrogen as Nitrates.	Ammonia.	Albuminoid Ammonia.	Oxygen, Absorbed in		Hardness, Clark's Scale, in degrees.		Total Solid Matter, dried at 220° Fahr.	No. of Colonies of Micro-organisms in 1 Gramme of the Water.
						Two Minutes at 80° Fahr.	Four Hours at 80° Fahr.	Before Boiling.	After Boiling.		
	Grains per gal.		Grains per gal.	Grains per gal.	Grains per gal.	Grains per gal.	Grains per gal.	Grains per gal.	Grains per gal.	Grains per gal.	
A deep well water supplying the County Asylum, Northampton, . .	1·8	None	·4981	·000	·0026	·0323	·0405	14·01	3·89	19·5	37
A well near Truro, Cornwall, ...	2·4	s. trace	·1153	·0006	·0052	·0646	·1107	2·32	·88	7·0	400
A surface well near Totness,	1·4	s. trace	·0101	·0014	·0006	·0215	·0827	5·56	2·9	13·0	457
Another well, also a surface well, near Totness, . .	1·6	...	·0067	·0004	·0017	·0397	·0963	5·21	3·20	12·5	394
A deep well in Hampshire, .	2·7	...	·6755	·0003	·0027	·0297	·0447	25·83	5·81	34·0	913

TABLE IV.

Examples of Bad Waters.

	Chlorine.	Phosphoric Acid.	Nitrogen as Nitrates.	Ammonia.	Albuminoid Ammonia.	Oxygen, Absorbed in		Hardness Clark's Scale, in degrees.		Total Solid Matter, dried at 220° Fahr.	No. of Colonies of Micro-organisms in 1 Gramme of the Water.
						Two Minutes at 80° Fahr.	Four Hours at 80° Fahr.	Before Boiling.	After Boiling.		
Water contaminated from being in a dirty cistern, .	2·8	s. trace	·671	·004	·0028	·0272	·0586	25·91	5·75	34·5	2,257
Water from a deep well near the sea shore, . .	23·0	trace	·0274	·0419	·0035	·0329	·0437	22·89	8·09	62·0	5,769
A surface well near Exeter, contaminated by dust and dirt, ...	2·15	trace	·2017	·0036	·0031	·1030	·1288	10·33	4·01	22·0	30,250
A well contaminated slightly by sewage in Essex, .	5·2	...	·4894	·0016	·0084	·0612	·1680	43·26	12-64	83·0	7,281
A well contaminated by sewage near Truro, Cornwall, ...	5·2	...	·7494	·0042	·0028	·0772	·0948	13·83	2·38	28·0	5,032

Monday, 4th January 1886.

THOMAS STEVENSON, Esq., M.Inst.C.E., President, in the Chair.

The following Communications were read:—

1. Notes on Experiments for the Board of Trade, made at the South Foreland Lighthouse by the Trinity House of London, on Lighthouse Illuminants, &c. By Thomas Stevenson, *Pres.* R.S.E., M.Inst.C.E.

A very important inquiry—suggested by Mr Chamberlain when President of the Board of Trade—has just been concluded by the Trinity House of London into the relative merits of electricity, gas, and oil, as lighthouse illuminants, on which I think it advisable to make a few remarks, as the general results of the investigation ought to possess a certain amount of interest for this Society.

Mr Wigham, gas engineer, Dublin, has long taken a great interest in the best means of increasing the power of our sea lights by means of gas and large burners. So far back as 1865 he proposed to increase the diameter of gas burners to 7 inches; and in 1868, the Scotch Lighthouse Board was asked by the Board of Trade to investigate the subject. Certain experiments were accordingly made at Granton in that and the following year. In 1869 the engineers of the Northern Lighthouse Board reported in the following terms, on the results of the experiments which had been made on the employment of large burners:—

"It has been found that the second series of experiments so far corroborated those previously made as to leave no room for doubt that the gas light when used with an annular lens, notwithstanding the greater size of the flame, was not superior to the effect of the smaller flame of the mechanical lamp, the explanation being that the greater portion of the large-sized 7-inch gas flame, consisting of 52 jets, is *ex-focal*, and is therefore lost, so that with the lens no advantage is gained by increasing the size of the flame beyond certain limits, and these seem to be pretty nearly attained in the ordinary 4-wick lamps. So apparent was this, that with Mr Wigham's concurrence, it was agreed to give up the idea of experimenting on the gas burner in its present form as applicable to revolving lights, and to confine the experiments to its use for fixed lights, in which, owing to the light being distributed equally over the horizon by the cylindric refractor, some advantage is gained by the employment of the larger flame of the gas." And the Report concludes as follows:—"We have to point out that the experiments made, have been highly valuable in showing the limit to which the size of a radiant may with advantage be increased when used in the focus of the apparatus now employed in lighthouses."

Mr Wigham, with great perseverance, nevertheless continued his labours in improving his large burners, and in producing new burners of still larger sizes. He also introduced several of these large-sized burners into the same lantern, in connection with Fresnel's 1st order lenses, so that from the same lantern he exhibited, under the name of biform, triform, and quadriform, two, three, and four burners, with a like number of Fresnel lenses, arranged one above another.

The employment of this multiple system of lights in the same lantern was, on the recommendation of Mr Wigham, adopted by the Irish Lighthouse Board, at Galley Head, in the county of Cork, in 1878. One important advantage of this multiple system was the power of lighting or extinguishing one or more of these burners, according to the greater or less amount of haze or fog in the atmosphere.

The Trinity House erected three experimental towers placed along side each other at the South Foreland Lighthouse, near Dover, and the photometric experiments were carried out by Mr Harold Dixon, M.A., of Balliol College, Oxford, while the officials of the Northern Lighthouse and Irish Lighthouse Boards were kindly invited to attend the experiments from time to time, as well as Mr A. G. Vernon Harcourt for the Board of Trade.

The conclusions which the Trinity House have arrived at are the following, and I may state, that in so far as my opportunities of observation have enabled me to form an opinion, I fully concur in their judgment:—

1. "That the electric light as exhibited at South Foreland has proved to be the most powerful light under all conditions of weather, and to have the greatest penetrative power in fog."

2. "That for all practical purposes the gas light, as exemplified by Mr Wigham's multiform system, and the oil light, as exemplified by the Trinity House Douglass 6-wick burners, in multiform arrangement up to triform when shown through revolving lenses are equal, light for light, in all conditions of weather; but that quadriform gas is a little better than triform oil."

3. "That when shown through fixed lenses, as arranged in the experimental towers, the superiority of the super-posed gas light is unquestionable. The larger diameter of the gas flames, and the lights being much nearer to each other in the gas lantern, give the beam a more compact and intense appearance than that issuing from the more widely separated oil burners."

4. "That for lighthouse illumination with gas, the Douglass patent gas burners are much more efficient and economical than the Wigham gas burners."

5. "That for the ordinary necessities of lighthouse illumination, mineral oil is the most suitable and economical illuminant, and that for salient headlands, important landfalls, and places where a very powerful light is required, electricity offers the greatest advantages."

There were employed during these experiments a variety of forms of photometer, but the most important was the Pentane standard photometer, the invention of Mr Vernon Harcourt, which was recommended to the Board of Trade, as giving the most uniform standard of comparison, by Professors Williamson and Odling, and Mr Livesey of London; while for outdoor observation, there was used a liquid photometer, consisting of a water-tight telescope, filled with a partially opaque fluid, supplied to the telescope from a small cistern fixed above, so that as the eyepiece is moved out or in, the length of the fluid through which the light has to pass increases or diminishes, and the length of the fluid through which the light so passes, is recorded by an attached graduated scale. This form of photometer was first proposed by me in the *Edinburgh New Phil. Journal* in 1863, and has since been employed in numerous experiments on lighthouse apparatus at Edinburgh.

The South Foreland experiments extended over a period of twelve months, and many interesting observations were made, besides those which related to the questions which were the more immediate cause of the investigations being instituted.

Electric Light.—The mean candle power of the electric arc given by one of De Meriten's machines, in a lamp with 40 mm. carbons, was found by Messrs Dixon and Harcourt to be 10,000 candles. It was found that the core carbons gave a better result than the solid carbons; also that two machines coupled do not give double the light of one; this result, according to the report of Professor Grylls Adams of Cambridge, was however due to the electric energy being expended in heating some of the "leads," which were of too small size. A considerable waste was also due to the regulator of the lamp.

Gas Burners.—The Wigham gas burners experimented with varied from 108 jets down to 28 jets, and from 2300 to 250 candle power; while the Douglass Argand gas burners varied from 6 to 10 rings, and their powers varied from 825 candle power to 2500.

Oil Burners.—The 6-wick Douglass oil burner was found to give 730 candle power, while another of similar construction with 9 wicks gave 1785 candle power.

These results show how successfully the effectiveness of lighthouse burners has been lately increased through the efforts of Mr Wigham and Sir James Douglass.

Variations in the Transparency of the Atmosphere.—Very remarkable variations in the transparency of the atmosphere were found to occur in the course of the observations, without any haze or fog or cloud being visible to the eye. Sometimes the experimental lights suffered a loss of $\frac{1}{4}$ to $\frac{1}{3}$ of their power, when inspected at a distance of a mile and a quarter; while the French lights on the other side of the Channel, 25 and 30 miles distant, continued to show with their usual steadiness and brilliancy without any variation.

The lenses employed in connection with the burners, which I have described were of two forms:—

1. The Mew Island lens, which is an ordinary Fresnel lens of the first order, 920 mm. focal distance.

2. The type employed at the New Eddystone Lighthouse, having the same focal distance, but with the addition of flint glass prisms above and below the central lens, so as to intercept a larger number of rays coming from the burner.

So far as the experiments at the South Foreland dealt with the optical instruments and burners *submitted for trial*, they may be held as conclusive and exhaustive. But in the experiments made in Edinburgh in 1869, it was pointed out to Mr Wigham, as a result of these experiments, that when his 52 gas jet burner (7 inches in diameter), was employed in combination with lenses suited for utilising the light from a flame of the diameter adopted originally by Fresnel, the effectiveness of this larger flame was to a great extent lost, as much of the light was *ex-focal*, thus escaping condensation by the Fresnel lens. It was not, however, until a comparatively recent date, that the matter of larger burners again came before Messrs Stevenson, as a question requiring settlement in practical lighthouse optics, when the Northern Lighthouse Board resolved to increase the size of the burners in a first order light in the service.

Provision had therefore to be made for utilising, as far as possible, the light from this increased size of burner, and in doing so it was decided, on the suggestion of Mr Alan Brebner, to test by actual trial whether Messrs Stevenson had been right in reporting that Fresnel's proportion insured the best results, and should not be violated.

An experimental lens in that proportion, suited for a 6-wick burner, was constructed and tried at the South Foreland, the focal distance of the lens being increased, while the holophotal system which I proposed in 1850, with totally reflecting prisms concentric with the refracting portion of the apparatus, was also adopted.

The lens as designed has a focal distance of 1330 mm., and has two reflecting prisms above and below the refracting portion, the whole subtending an angle of 60° horizontally and of 70° vertically.

A panel of this design renders unnecessary the use of special flint glass refractors, such as are employed in the New Eddystone apparatus, and also prevents the loss of light due to the square form of the Fresnel lens.

After the experiments on electricity, gas, and oil were concluded at the South Foreland, this lens, before being used in the Northern Lighthouse service, was kindly allowed to be tested by comparative trials. This was done by Sir James Douglas and Mr D. A Stevenson by comparing the depths of the shadows thrown by the different apparatus, and by exact photometric observations made by Mr Dixon, of which the following are the results :—

Time.	No. of Burner.	Lens.	Illuminating Power in Pyres or 1000 Candles.	Rate of Gas Consumption.
7.30	I. 108 Gas	Mew Island	22·8	300
7.40	I. 6-wick Oil	Stevenson	29·3	...
7.48	I. 108 Gas	Mew Island	23·4	300
7.55	I. 6-wick Oil	Eddystone	18·1	...
8.	II. 108 Gas	Mew Island	41·6	298
8.8	I. 10-ring Gas	Eddystone	31·8	225
8.15	I. 10-ring Gas	Stevenson	62·2	...
8.20	II. 108 Gas	Mew Island	43·2	298
8.25	High Light	...	7·6	...
8.30	I. 108 Gas	Mew Island	23·1	310
8.35	I. 6-ring Gas	Stevenson	28·9	100
8.50	I. 6-ring Gas	Eddystone	13·7	110
8.55	I. 108 Gas	Mew Island	21·	310
9.15	I. 108 Gas	Mew Island	22·4	310
9.20	I. 108 Gas	Stevenson	42·6	330
9.25	I. 108 Gas	Mew Island	22·4	310

The following conclusions from these photometrical trials seem warranted :—

1. That a single large burner shown in a complete panel of a revolving apparatus composed of the new lens, with totally reflecting top and bottom prisms of corresponding size added, would give a more intense light than burners and ordinary Fresnel lenses, arranged as biform and equal to triform.

2. That the consumpt of oil or gas would be a $\frac{1}{2}$, $\frac{1}{3}$, and $\frac{1}{4}$ of that of biform, triform, or quadriform arrangements respectively.

3. That the new lens apparatus would avoid all the disadvantages of superposed lenses, including excessive heat in the lightroom, difficulty of management of the burners, and obstruction of light by the necessary ventilating tubes.

The practical result is obvious, that for lighthouse purposes, on the grounds of superior illuminating power, economy and ease of management, the principle of increasing the diameter of the apparatus in proportion to the size of the burner, is superior to that of superposing lenses and burners; in a word, that the new lens apparatus is superior to the biform, triform, and quadriform arrangements.

The trials at the South Foreland were made with apparatus in existence at the time when the experiments were instituted, and I have since proposed to take advantage of the natural properties of the electric light, by adapting to it more fully the condensing system, now largely employed on the Scottish coast, both for flashing and fixed lights. This system has been employed for the new electric group flashing light for the Isle of May, in connection also with a mode which I proposed of dipping the rays below the horizon on the occurrence of haze or fog, so as at such times to increase the power of the light at short distances from the lighthouse, where alone it is likely to be seen in such states of the atmosphere.

In the apparatus experimented with at the South Foreland by the Trinity House, the condensing principle was also applied, but to a very small extent, viz., the condensation of 30° into 5° or 6 times, while in the Isle of May apparatus the light proceeding from 45° is condensed into 3° or 15 times, and the emergent beam admits of being dipped to any extent which may be found necessary during fogs.

The peculiar property of this azimuthal condensing arrangement is that, where the light is intermittent, the power is increased in proportion to the intervening periods of darkness. Thus, neglecting the loss of light by absorption &c., the power is *doubled* when the periods of light and darkness are equal, *trebled* when the dark periods are made twice as long as the light, and so on in proportion, while in every case the rays are spread uniformly over each illuminated sector.

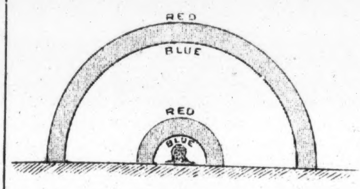
2. Glories, Halos, and Coronæ seen from Ben Nevis Observatory. Extracts from Log Book. By R. T. Omond. Communicated by Professor Tait. (Plate XVI.)

Dec. 1, 1883.—Glory seen at 9^h 15^m (see fig. 1). Red outside in both rings. Smaller one almost filled up by shadow of head. Radius of outer red = 2°. At 11^h 12^m and 12^h 0^m *white* fog bow. When very bright it was coloured red outside. Too large to measure.

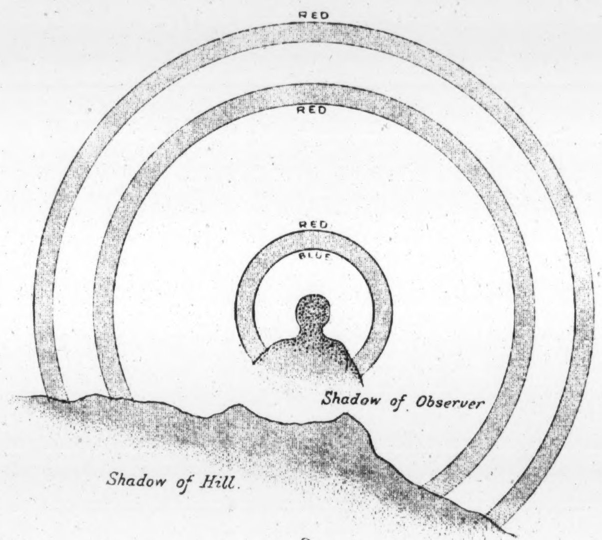
Dec. 6, 1883.—Corona round moon all evening. Red outside. Radius of red, 2° 8′. Occasionally an outer circle of a bluish colour, extending from red ring outwards to a radius of 4°, was visible.

Bluish
Red
Yellow
Blue
☽

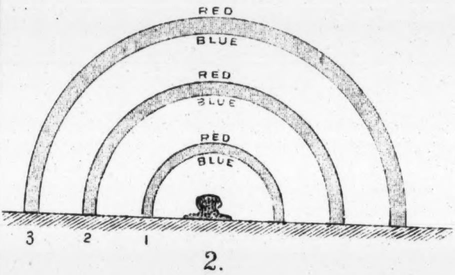
Dec. 17, 1883.—Corona at 2^h. Double ring, red outside in both. At 22^h corona again. Triple ring, red outside in all three. Radius of middle red = 1° 45′. At same time lunar


RED
BLUE
RED
BLUE

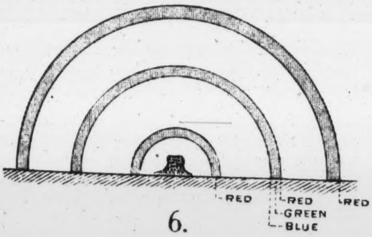
1.


RED
RED
RED
BLUE
Shadow of Observer
Shadow of Hill.

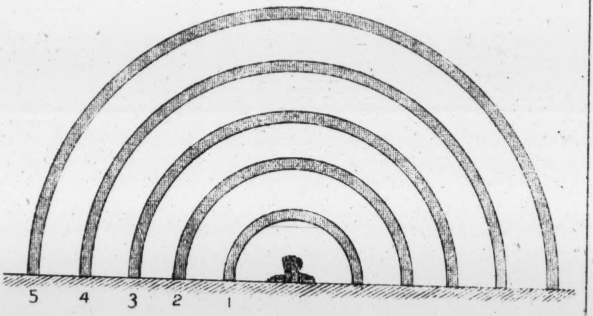
3.


RED
BLUE
RED
BLUE
RED
BLUE
3
2
1

2.


RED
RED
GREEN
BLUE
RED

6.



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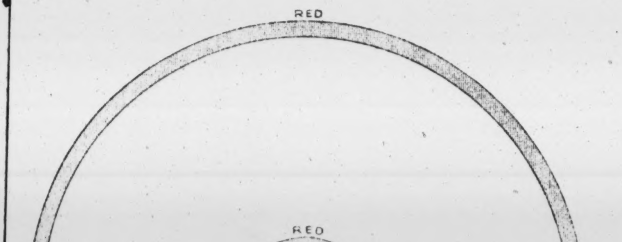
RED
RED

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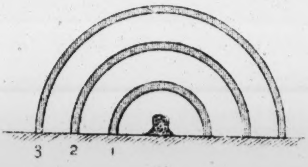

5.

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MUSEUM

rainbow, radius 26°, colours if any very faint. Lunar fog bow a 23^h. Too large to measure.

Dec. 18, 1883.—Badly defined corona at 6^h, red outside. Radius of red about 2°.

Dec. 23, 1883.—At 12^h 30^m reddish hue round sun; radius about 7° or 8°. At 15^h 10^m sun sank into cloud bank. As it went down semicircle of colours formed over edge of cloud bank; red outside, like badly-defined corona. Sky above covered with detached stratus clouds, and not coloured at all.

Dec. 25, 1883.—Fleeting, badly defined glories seen all afternoon on fog in northern valley. Red always outside.

Jan. 3, 1884.—At 17^h 35^m, lunar halo, white, very faint. Diameter 17° (*sic*).

Jan. 15, 1884.—At 1^h lunar corona seen. Distinct orange ring, 2° 30′ radius, inside of which is the true corona, with blue next moon. Outside orange ring a bluish green space edged with red or orange rim. This space did not look like a second corona; the colour was too uniform. Radius of outer red ring, 5°. This green in the outer ring strongly resembled the green tints seen in the recent sunsets. At 2^h the moon was surrounded by a very faint corona entirely blue.

Red or
Orange
Bluish
green
Orange
☽

> *Note.*—This was found afterwards to be a common type of corona.

March 4, 1884.—At 10^h 50^m, solar halo observed. Red inside. Colours very faint; approximate radius = 22°. At 20^h lunar halo observed, white, very faint; approximate radius, 23° 45′. At 21^h corona observed. Double. Usual colours out to first red ring; then bluish annular space tinged with red outside. Radius of first red, 2°; radius of second red, 4°.

Faint red
Bluish
Red
☽

April 7, 1884.—At 3^h lunar halo observed; not well defined, white. Radius by stef., 22° to 23°.

April 8, 1884.—At 23^h blue lunar corona observed. Radius (of red) about 4° 25′. Same corona seen till 3^h on April 9.

Faint red
Blue
☽

April 10, 1884.—At 2^h lunar corona observed. Inner space blue; then green ring, and red ring outside all. Radius of red, 4° 15′.

Red
Green
Blue
☽

April 13, 1884.—At 4^h lunar corona observed, very misty looking, with red ring inside an outer green one.

Green
Red
☽

N.B.—Probably usual colours inside red, though not noted in log.

Red
Green
Reddish yellow
☽

May 3, 1884.—At midnight brilliant lunar corona observed; colours as in diagram. It disappeared suddenly at 0^h 5^m before there was time to measure it.

May 6, 1884.—Badly defined corona seen at midnight. Radius of red ring 3° 15′, and of blue 2°.

May 11, 1884.—Solar halo observed at 10^h. Well defined. Red inside. Radius, 22° 30′.

May 23, 1884.—Solar halo at 13^h. Radius about 20°. Colours very brilliant. Red inside, blue outside.

Blue
Red
☽

June 8, 1884.—At midnight lunar corona seen. Red outside, with blue margin beyond. Radius of red, 2° 30′. Very misty; size apparently varying.

Faint red
Blue
Red
Yellow
Blue
☽

June 9, 1884.—At 1^h double lunar corona observed; colours as in diagram. The inner one had all the gradations of spectroscopic colours, the outer only blue and faint red. Radius of inner red, 2°; radius of outer red, 4°. At 2^h inner ring as before, but no outer colours.

June 17, 1884.—At 19^h double and triple glories seen. Colours as noted in fig. 2. No. 1 was very indistinct—a mere blotch of colour. No. 2 was the brightest; No. 3 was not so bright as No. 2, and sometimes was not seen, but when visible was quite sharp and distinct:—

Measured by	Red (2)	Blue (2)	Red (3)
R. T. Omond, . .	1° 55′	1° 50′	3° 10′
A. Rankin, . .	1° 55′	...	3° 30′

June 18, 1884.—At 8^h double fog bows seen, with faint glory round the shadow of observer. About $\frac{3}{4}$ of the circles were visible (270°), the rest being cut off by the shadow of the hill. Colours as noted in fig. 3. The inner (fog) bow was narrower, and not so distinctly coloured as the outer. Both were too large to measure with stephanome.

July 26, 1884.—At 10^h solar halo observed; colours quite distinct, red inside.

Radius of red, . .	1st measurement,	22° 45′
	2nd ,,	22° 30′
	3rd ,,	23° 0′
Mean 22° 54′.	4th ,,	23° 20′

August 12, 1884.—At 3^{h} badly defined lunar corona, red outside. Radius of red, 2° 50′.

August 31, 1884.—At 21^{h} 12^{m} double lunar corona and a lunar fog bow observed. Colours of corona as in diagram. Radius of outer red, 4° 32′; radius of inner red, 2° 15′.

Red
Yellow
Green
Blue
Red
Yellow
White
☽

Sept. 11, 1884.—At 1^{h} lunar corona observed. Radius of red ring, 3° 22′. Yellow inside the red, and outside the red a blue margin.

Blue
Red
Yellow
☽

Sept. 17, 1884.—In the afternoon glories were seen on the fog in the valley to northward. Red outside, with yellow, green, and blue inwards. At times a faint outer (second) one was seen, but they were all too evanescent for measurement.

Similar glories were seen on Sept. 20.

Oct. 4, 1884.—While the moon was partially eclipsed a strong double corona appeared round it. Radius of outer red, 3° 53′; radius of inner red, 2° 26′.

Oct. 5, 1884.—At 1^{h} 15^{m} double fog bow observed. Radii approximately $37\frac{1}{2}$° and $32\frac{1}{2}$°. At 1^{h} 30^{m} triple corona observed. Radii: 1st ring (outside of red), 2° 3′; 2nd (outside of red), 3° 1′; 3rd (outside of red), 4° 5′.

N.B.—For details of the last two coronæ see *Nature*, Oct. 23, 1885, vol. xxx. p. 613.

Oct. 12, 1884.—Lunar corona at 5^{h}. Colours as in diagram. The outer blue in coronæ of this type forms a distinct margin or *glare*. This corona was formed on passing fog or scud; its size appeared to vary.

Blue
Red
Blue
☽

Nov. 3, 1884.—At 11^{h} solar fog bow observed, with colours; red inside.

Nov. 7, 1884.—At midnight faint misty corona observed; red outside. Radius of red = 2° 30′.

Blue
Red
White
☽

Nov. 13, 1884.—At 6^h faint corona seen; no distinct colours, but a faint red ring with white inside it and blue outside as in diagram. At noon glory and fog bow seen in valley to northward.

Nov. 23, 1884.—From 9^h onwards glories observed on fog to northward, apparently about 2 miles distant. The number of rings varied from 3 to 5 (see fig. 4). At 14^h measurements were taken—1st red, too small to measure (under 1° 40′); 2nd red, 2° 0′ radius; 3rd red, 6° 30′; 4th red, 9° 40′; 5th red, too faint to measure. The 1st ring was blurred and indistinct looking; the 5th was incomplete and very faint; in each ring red outside and blue inside.

Blue
Red
Yellow
☽

Blue
Red
☽

Nov. 29, 1884.—At 2^h small lunar corona seen. Very yellow looking inside the red, and with blue margin outside. Radius of red, 1° 55′. At 22^h corona as in (lower) diagram. Colours inside the red not distinct. The outer blue not always seen. Radius of red, 1° 55′.

Blue
Red
Blue
☽

Dec. 1, 1884.—(At 1^h?) Corona with red outside but blue margin formed on scud passing over the hill. Radius of red = 4° 0′. Space inside the red blue looking. When the scud cleared off at any time the corona disappeared, but the moon was seen surrounded by a very much smaller one, very yellow-looking, inside the red. At 3^h, misty-looking corona formed on scud; red outside. Radius of red = 2° 52′.

Dec. 4, 1884.—At 1^h, corona with same arrangement of colours as in diagram above (Dec. 1), but the blue margin only seen at times. Radius of red = 2° 0′.

Red
Yellow
and green
☽

Dec. 25, 1884.—At 17^h lunar corona observed, very misty-looking. Radius of red, 2° to 2½°. Very green and yellow in appearance; no blue, and red faint. At 20^h very faint blue corona round moon, no other colour visible; radius of this blue space about 2° 5′. At 22^h small corona. Red too small to measure (under 1° 40′). Blue extending to about 3° 10′ radius. At midnight corona similar to that at 22^h, but without the outer blue margin. Radius of red = 2° 0′.

Blue
Red
Yellow
☽

Dec. 27, 1884.—At 18^{h} lunar corona observed; colours as in diagram. Radius of blue^{2} = 1° 50′; radius of red^{2} = 2° 25′; radius of red^{3} = 4° 30′. At 20^{h} lunar corona, with colours as in diagram. Outer red, rather faint. Radius of red^{1}, 2° 2′; radius of red^{2}, 4° 5′. At 21^{h} double corona still seen. Its size appeared to vary, being small when the sky was clear of low clouds, and enlarging as the scud passing over the hill thickened.

Red^{2}
Blue^{3}
Red^{2}
Blue^{2}
Red^{1}
Whitish
☽

Red^{2}
Blue
Red^{1}
Yellow
☽

Dec. 28, 1884.—At 21^{h} moon was surrounded by a white *glare* about 5° radius; no halo or corona. At 23^{h} and midnight, moon surrounded by blue corona. No other colours visible; outside radius, 3° 25′. At the same time the white glare had increased to about 15° radius.

Dec. 29, 1884.—At 20^{h} single corona; red outside. Radius of red = 3° 28′. At 23^{h} double corona; colours as in diagram; outer red very faint; rather misty-looking; the size appeared to vary. Two measurements of the radius of the inner red (red^{1}) gave 3° 40′ and 3° 55.′

Red^{2}
Blue
Red^{1}
Blue and
Yellow
☽

Jan. 24, 1885.—At 22^{h} faint misty lunar corona. Red outside. Radius of red = 2° 30′.

Jan. 29, 1885.—At 4^{h} misty lunar corona; colours as in diagram. Outer red very faint, making the blue look more like the *margin* often seen than a part of the corona proper. Radius of inner red, 2° 14′; radius of outer red, 3° 38′. At 21^{h} faint blue corona on scud.

Red^{2}
Blue
Red^{1}
Yellow
☽

Feb. 4, 1885.—At 7^{h} 5^{m} double corona observed; colours as in diagram. Radius of inner red, 2° 36′; radius of outer red, 5° 10′.

Red^{2}
Blue
Red^{1}
Whitespace
☽

Feb. 19, 1885.—At 21^{h} lunar corona observed; colours as in diagram. Radius of red, 2° 30′.

Blue
Red
Yellowish
☽

Feb. 20, 1885.—At 21^{h} small yellow lunar corona with blue margin, the margin being faintly tinged with red (outside). Radius of this red = 4° 3′. [Really a double corona with the inner red cut out.]

Red
Blue
Yellow
☽

Feb. 23, 1885.—At 19^{h} faint lunar corona formed on upper clouds. One red ring, rather broad. Radius of inside of this red ring = 1° 45′; radius of outside of this red ring = 2° 40′.

Bluish pink
Yellow
Pink to Violet
Blue
Yellow (pale)
Whitish yellow
✳

Feb. 28, 1885.—Solar corona seen by Mr Rankin from Plateau of Storms at about 16^h 15^m. Formed on scud. Colours as in diagram. Outside these portions of circles were seen flashing for a short time, but their colour was not determined.

Red
Orange
Greenish blue
Red
Yellow
White
☽

March 1, 1885.—At 3^h lunar corona; colours as in diagram. Formed on scud. Radius of inner red = 3° 18′; radius of outer red = 4° 49′. Solar halo at 14^h 23^m; faint red inside; radius by steph. about 18°.

March 21, 1885.—At 20^h lunar halo observed. Radius by stephanome = 23° 15′.

Red
Blue
Red
Watery yellow
☽

March 22, 1885.—At 20^h double lunar corona; colours as in diagram. Radius of inner red, 1° 53′; radius of outer red, 4° 40′. It was formed on passing cloud, but when the sky was clear a faint blue *glare* surrounded the moon.

March 30, 1885.—At 22^h lunar halo on cirrus clouds. Radius by stephanome = 18°.

April 8, 1885.—Solar halo at 10^h and 11^h. Red inside. Radius by stephanome (doubtful observation), 25° 50′. At 11^h the top and bottom of this halo were brighter than the sides.

April 17, 1885.—At 6^h faint solar halo, which vanished at 6^h 10^m, except two very bright spots with red towards sun, one on each side of sun, and at the same height above the horizon. Radius by stephanome, 24°. At 7^h faint halo was again observed; at 8^h the highest part appeared brightest, and at 9^h only that part remained visible. At 13^h the halo was again visible, and five measurements were made as under:—

$$\left.\begin{array}{lcl}\text{1st radius} & = & 22^\circ\ 45' \\ \text{2nd}\quad ,, & = & 23^\circ\ 40' \\ \text{3rd}\quad ,, & = & 22^\circ\ 15' \\ \text{4th}\quad ,, & = & 23^\circ\ \ 0' \\ \text{5th}\quad ,, & = & 22^\circ\ 15'\end{array}\right\}\text{To inside of red ring.}$$

At 14^{h} upper part only visible, with colours as shown in diagram.

$$\text{Radius of red,}\begin{cases}23^\circ\ \ 0'\\24^\circ\ 15'\\23^\circ\ \ 0'\end{cases}$$

Bluish white
Yellow
Red
Bluish purple
✱

July 3, 1885.—At 1^{h} and 2^{h} small misty lunar corona observed; colours as in diagram, with a very faint blue margin outside red. Radius of red ring = 1° 52′.

Red
Greenish yellow
☽

July 18, 1885.—At 6^{h} solar halo observed. Radius by stephanome, 22° 48′. Rather indistinct.

July 19, 1885.—At 9^{h} solar corona observed; coloured as in diagram.

Faint white
Strong white
Faint red
Pink
✱

July 24, 1885.—Solar halo at 17^{h}. Radius by stephanome (mean of 6 readings), 22° 27′.

August 2, 1885.—At 3^{h} misty lunar corona; colours rather blurred; red outside; radius of red, 3° 16′. At 11^{h}, glories on fog in valley to northward. Radius by stephanome, 3° 36′. [Probably red ring.]

August 22, 1885.—At 23^{h}, misty corona; colours as in diagram, the blue margin being well marked. Radius of red about 1° 50′.

Blue
Red
Yellow
☽

August 25, 1885.—At 1^{h} double lunar corona observed; colours as in diagram. Radius of outer red about 9° 4′; radius of inner red about 5° 14′; radius of yellow about 3° 36′. Too misty-looking to measure accurately. [A similar corona had been seen on the two previous nights, but was not measured.]

Red
Blue
Red
Yellow
Whitish
☽

Oct. 20, 1885.—At 22^{h} double lunar corona, with faint outer blue margin, formed on fog passing across the hill top; colours as in diagram. Radius of inner red, 1° 45′; radius of outer red, 3° 52′.

Faint blue
Red
Green
Red
Yellowish white
☽

Oct. 22, 1885.—Double lunar corona observed at 19^{h}; colours as in diagram. Radius of outer red, 10° 53′; radius of green and blue, 6° 55′; radius of purple, 4° 40′; radius of inner red, 3° 42′; radius of white, 2° 25′. Similar corona observed at midnight;

Red
Green and Blue
Purple
Yellowish red
White
☽

Red
Yellow
Green
Red
Yellowish white
☽

colours as in diagram; the (outer) yellow a very narrow strip. Occasionally traces of a third ring were seen. Radius of outer red, 4° 56′; radius of inner red, 2° 11′; radius of yellow, 4° 23′. All good measurements.

Oct. 23, 1885.—Lunar corona at 2^h, similar to that at midnight on 22nd, except that the inner yellow was more distinctly marked. No blue or green inside the inner red. Yellows, both outer and inner, only thin strips; reds and green broader than, and not so sharply defined as, the yellows. Radius of inner red, 2° 29′; radius of outer red, 4° 43′; radius of inner yellow, 2° 4′; radius of outer yellow, 4° 8′.

Oct. 23, 1885.—Lunar halo, colourless, seen at 3^h. Radius to inside of halo, 21° 48′; radius to outside of halo, 25° 13′. The halo was somewhat indistinct, and the measurements are rather rough.

Red
Yellow
Bluish green
Red
Bluish white
☽

At midnight double corona formed on scud. The size varied according to the sort of cloud or fog it was formed on. The following measurements were taken within three minutes of each other [colours as in diagram]—1st radius of inner red, 2° 14′; 1st radius of outer red, 4° 18′; 2nd radius of inner red, 2° 50′; 2nd radius of outer red, 4° 49′.

Red
Yellow
Green
Red
Yellow
Watery white
☽

Oct. 25, 1885.—At 4^h double lunar corona with traces of third ring; colours as in diagram. As before, the yellows were narrow and the green broad. Radius of inner red, 2° 10′; radius of outer red, 4° 20′; radius of inner yellow, 1° 45′; radius of outer yellow, 2° 50′.

Nov. 11, 1885.—At 9^h glory, with only one broad red ring observed. Radius to inside of ring, 1° 44′; radius to outside of ring, 2° 21′. At 15^h glory with three rings, as in fig. 5; the innermost a mere blotch, the next a well-marked ring with all the colours; the outermost the same, but fainter. Radius of red^3, 3° 46′; radius of red 2^2, 1° 52′. [Red outside in both rings.]

Nov. 16, 1885.—At 9^h glory seen on passing fog; colours as in fig. 6. The inner red close to the shadow of the observer's head. Radius of red^2, 7° 38′; red^3, too faint to measure.

Nov. 18, 1885.—At 23^h double lunar corona, red outside. The size varied very much; at times the two sets of colours merged into one misty corona which shrank rapidly in, a new second set forming outside as the former one contracted.

Nov. 22, 1885.—Lunar corona formed on high cirro-cumulus clouds at 19^h and 20^h; colours as in diagram, but very badly defined. At 19^h red very broad. Radius to inside of this red, 1° 54′; radius to outside of this red, 4° 18′. At 20^h colours better marked a little, but still vague. Radius of red, 2° 54′; radius of yellow, 2° 35′; radius of outside of blue, 1° 42′. Blue much broader than the red or yellow, and with no defined inner margin.

Pale red
Yellow
Bluish
white
☽

Nov. 23, 1885.—Double lunar corona on cirro-cumulus at 4^h. Badly defined, but coloured as in diagram. Radius of outer red, 6° 48′; radius of inner red, 3° 7′; radius of yellow, 2° 42′. Yellow a narrow line; green a broad belt.

Red
Green
Red
Yellow
Bluish
☽

Dec. 16, 1885.—Double lunar corona at 20^h; colours as in diagram; the outer yellow very faint. Formed on scud; no corona visible when scud clears off. Radius of outer red, 3° 48′; radius of inner red, 1° 58′; radius of outer yellow, 3° 38′; radius of inner yellow, less than 1° 40′. Similar corona at 22^h. When the scud got very thick the outer part was more yellow and less blue looking. Radius of outer red, 3° 33′; radius of inner red, 1° 53′; radius of outer yellow, 3° 0′; radius of inner yellow, 1° 23′. Two minutes after—outer red, 3° 8′; inner red, 1° 42′. Very faint traces of third ring at times.

Red
Yellow
Blue green
Red
Yellow
Watery
yellow
☽

Dec. 18, 1885.—At 11^h solar halo, white. Very faint except in one spot to eastward of sun (horizontally). Radius = 23° 3′.

Dec. 19, 1885.—At 3^h faint blue lunar corona; radius, 3° 4′. The edge of this corona was the brightest part, but the whole space inside was blue, and no other colour was visible. At 4^h similar corona, but with very faint red edge outside. Radius = 3° 20′. At 17^h lunar corona; red outside. Radius to outside of red = 4° 13′; to inside of blue, 2° 55′.

Red
Yellow
Blue
Red
Pale yellow
Bluish white
☽

Dec. 20, 1885.—Double lunar corona at midnight; colours as in diagram, but very pale and faint. Radius of inner red, 3° 3′.

Dec. 21, 1885.—At 5^h triple lunar corona. Reds outside. Radius of innermost red, 1° 5′; radius of middle red, 1° 55′; radius of outermost red, 2° 55′.

Dec. 22, 1885.—Lunar halo, white, at 23^h. Radius to inside, 20° 41′ and 20° 16′; radius to outside, 27° 29′ and 27° 23′. Two measurements.

Red
Yellow
Blue
Red
Yellow
Pale green
Red
Yellow
Bluish yellow
☽

Dec. 23, 1885.—Triple corona at 4^h; colours as in diagram, but outermost ring very faint. Radius of innermost red, 1° 35′; radius of innermost yellow, 1° 11′; radius of outside of green, 2° 26′; radius of middle yellow, 2° 48′; radius of middle red, 3° 12.

Red
Yellow
Yellowish
☽

Jan. 18, 1886.—At 17^h double lunar corona observed; colours as in diagram. Radius of red, 1° 41′; radius of yellow, 0° 58′. Similar corona seen at 21^h.

Red
Yellow
Bluish green
Red
Orange
Yellow
Whitish
☽

Jan. 20, 1886.—Double lunar corona observed at 2^h. Formed on scud. Colours as in diagram. . Very fleeting, and size varying. Two measurements made within one minute of each other gave for radius of inner red, 3° 33′ and 3° 7′. The colours were always the same, though the size varied.

Feb. 10, 1886.—Double solar fog bow seen at 11^h. Traces of red on outside of outer bow, and on inside of inner. Radius of outer bow about 42° 4 ; radius of inner bow about 33° 0′.

Feb. 14, 1886.—At 11^h glory and fog bow seen on fog in northern valley (see fig. 7). Fog bow tinged with red outside; outside of glory red also. Outer radius of fog bow, 45° 12′; inner radius, 40° 0′; radius of *red* of glory, 1° 6′.

Feb. 15, 1886.—At 9^h solar halo, with portion of reverse arc on upper side of it *in large* halo, red inside and blue outside; in portion above, red next sun and blue inside curvature. This upper arc overlapped on the main circle, bringing their reds together.

Radius from centre of sun to point where the two reds overlapped,	21° 42′ 21° 49′ 22° 0′ 22° 0′ 22° 0′ 22° 0′

The upper arc was very distinct, but the blue in it was fainter than the blue of the main circle. The radius of upper arc was not determined. Double solar corona at 11ʰ. Radius of outer red, 40° 53′; radius of inner red, 2° 55′. From 19ʰ to 23ʰ lunar halo on cirrus clouds, and for most of the time misty corona also on cirrus. At 19ʰ the following measurements were made:—

Corona.		Halo.	
Outside radius, . .	4° 20′	Radius, . . .	24° 28
,, ,, . .	4° 28′	,, . . .	23° 17′
This corona was a mere bluish space, with a red rim on outside edge.		,, . . .	22° 24′
		,, . . .	22° 36′
		,, . . .	23° 30′
		,, . . .	23° 44′

At 20ʰ the measurements got were—Radius of corona, 2° 59′; halo, 21° 13′.

Feb. 16, 1886.—At 2ʰ, misty-looking lunar corona on cirro-cumulus; colours as in diagram. Radius of red, 1° 43′.

Faint blue
Red
Yellow
☽

Feb. 17, 1886.—At 3ʰ, blue corona round moon. Misty corona on cumulus at night; no measurement made.

Feb. 25, 1885.—Lunar corona at 5ʰ. Colours were in the following order out from moon:—

☽ White. Yellow. Red. Violet. Green. Yellow. Red.

Feb. 26, 1886.—At 12ʰ solar corona. Double rings rapidly varying in size. Radius of outer red, 8° 22′; radius of inner red (3 measurements), 4° 34′; 4° 23′; 3° 38′. Similar corona at 13ʰ, but outer ring very faint. Radius of inner red, 3° 3′; 2° 47′; 3° 25; 3° 10′; 3° 2′; 2° 24′; radius of outer red, about 5° 30′.

March 5, 1886.—At 8ʰ fog bow; corona (3, 4, and 5 rings) and glory (2 and 3 rings) on passing fog,—all too fleeting to measure.

March 7, 1886.—At 7^{h} thin pallium over sky just above the level of the hill tops round. At 7^{h} 13^{m} the sun broke through this cloud layer, and at 7^{h} 16^{m} a white beam was seen stretching vertically downwards from sun to edge of Ben Nevis (to eastward). It passed through a stratus cloud. Corona observed at the same time. At 7^{h} 24^{m} the beam was again seen. At 10^{h} double solar corona was observed. The following measurements were made:—

Radius of inner red,	3° 46′	Radius of outer red,	7° 30′
,, ,,	3° 37′	,, ,,	5° 34′
,, ,,	3° 30′	,, ,,	6° 29′
,, ,,	4° 1′	,, ,,	6° 35′
,, ,,	3° 42′		
,, ,,	3° 42′		
,, ,,	2° 50′		

N.B.—The last measurement of the inner red and the second of the outer were taken about the same time.

Solar halo observed at 11^{h}, at 12^{h}, and again at 13^{h}. The following measurements were made:—

	At 11^{h}.	At 12^{h}.	At 13^{h}.	
Radius, . .	23° 30′	23° 44′	21° 48′	22° 24′
,, . .	22° 0′	21° 13′	21° 24′	23° 3′
Extreme inside,	21° 13′	22° 12′	22° 24′	21° 48′
,, outside,	25° 44′		21° 13′	22° 0′
			22° 12′	22° 12′

March 13, 1886.—Fleeting lunar corona, sometimes single and sometimes double, formed on scud at night.

NOTES.

Whether specially mentioned or not, all the measurements were made by the stephanome. Up to and including that on July 3, 1885, the sliding rings (1½-inch and 8-inch) were used; after that the sliding bar with points on it.

The measurements made with the bar are considerably more accurate than those with the rings, the bar being more easily handled, and the points capable of more accurate adjustment to the object observed.

The type of corona seen on August 22, 1885, is of very common

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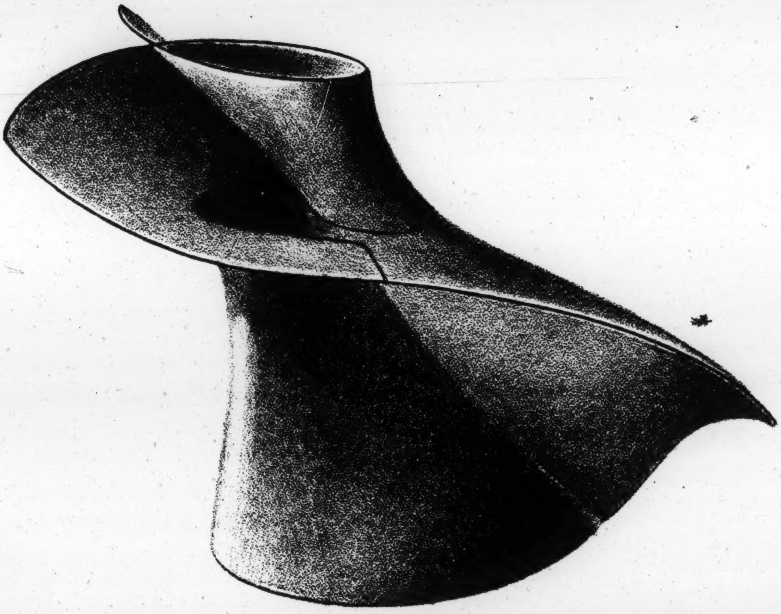
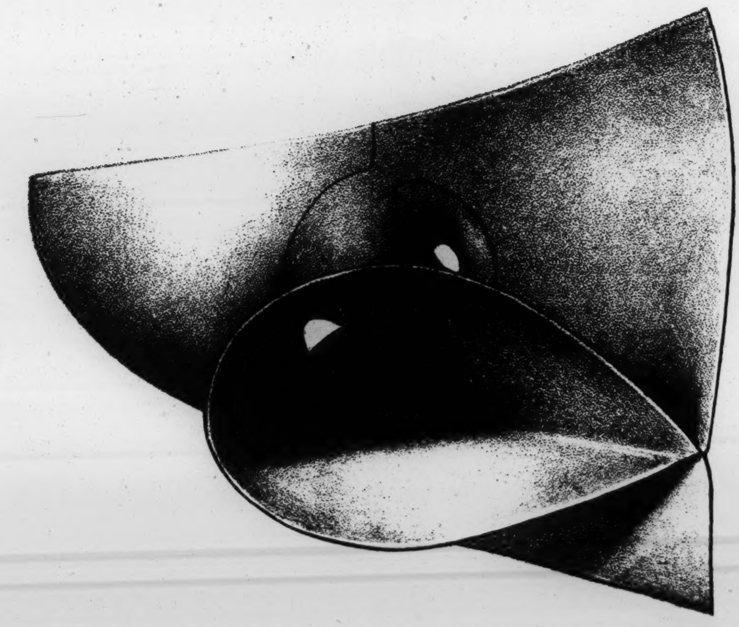
MODEL OF THE HALF-TWIST SURFACE.





occurrence. When there are two complete sets of colours, the outer is always green or blue looking, and the inner yellow.

The measurements of halos may serve to show the limits of error of the instrument.

3. On a Model of the "Half-Twist Surface." By Professor Crum Brown. (Plate XVII.)

The "twist surfaces," of which this is a case, stand in the same relation to the helicoid surface as the anchor-ring does to the cylinder. In the helicoid the generating line, at right angles to the axis, rotates about the axis as the point of intersection moves along it. In the twist surfaces the generating line is always at right angles to a fixed circle, and rotates about the tangent to the circle at the point of intersection, as the point of intersection moves round the circle. The species of twist surface is defined by the ratio of the angular motion of the generating line to that of the point of intersection. In the particular case illustrated by the model, the generating line turns through two right angles, while the point of intersection makes one whole revolution; that is, the rate of angular motion of the generating line is one-half of that of the point of intersection.

The idea of making such a model was derived from the "one-sided surfaces" exhibited by Professor Tait, formed by gumming together the ends of a strip of paper, after giving it half a turn about its axis. Such a strip has only one side and only one edge, or, perhaps more accurately, its two sides are continuous, and its two edges are continuous. If such a strip is very narrow, and if it is so arranged that its central line is a circle, it may be considered as a portion of a "half-twist" surface. Without entering into any detailed mathematical discussion of the surface, there are some points of interest which may be indicated. A straight line passing through the centre of the circle and at right angles to its plane, obviously lies wholly in the surface, as every generating line cuts it. We may call this line the axis of the surface. Every plane through this axis contains two generating lines; the intersections of these pairs of generating lines lie in a straight line touching the circle, and inclined at an angle of $\frac{\pi}{4}$ to its plane. The surface therefore intersects itself in this straight line. It is obvious that the surface has "helicoid asymmetry"; as for each sense in which the point of intersection may rotate, there are two senses in which the generating line may rotate. This gives four forms, which obviously coincide in pairs.

4. On the Linear Section PR of a Knot M_n, which passes through two Crossings P and R, which meets no Edge, and which cuts away a $(3+r)$-gonal Mesh of M_n. By Rev. Thomas P. Kirkman, M.A., F.R.S.

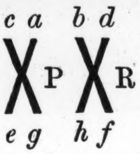
1. The purpose of this paper is to give rules for the construction of knots M_n of n crossings, having each one or more linear sections PR, such that by unkissing at P and at R, shall be obtained two unifilar knots of $n-1$ crossings. See my paper "On the Twists of Listing and Tait," page 363 of the *Proc. R.S.E.* The knot M_n is always made by a section *ffc*, *i.e.*, by uniting the P's and R's of two 2-gons (PR) on K_{e+2} and L_{n-e}, after cutting away the four edges PR. These 2-gons can only be (a bifilar 2-gon has edges in two circles)—

(*a*) Bifilars found on bifilars K and L;
(*b*) Bifilar found on bifilar K, and even on unifilar L;
(*c*) Odd laid on even of unifilars K and L;
(*d*) Odd laid on odd of unifilars K and L;
(*e*) Even laid on even of unifilars K and L; or
(*f*) Bifilar of bifilar K laid on odd of unifilar L.

2. (*a*) *Bifilar on Bifilar.*—Let the pairs (A) and (B)

$$\begin{matrix} \ldots a\mathrm{PR}b\ldots \\ \ldots c\mathrm{PR}d\ldots \end{matrix}\ (\mathrm{A}) \qquad \begin{matrix} \ldots e\mathrm{PR}f\ldots \\ \ldots g\mathrm{PR}h\ldots \end{matrix}\ (\mathrm{B})$$

be the circles of the bifilar knots K_{e+2} and L_{n-e}. In (A) the angles $a\mathrm{P}c$ and RPR are covertical, as are $b\mathrm{R}d$ and PRP. In (B) $e\mathrm{P}g$ and RPR are covertical, and also $f\mathrm{R}h$ and PRP. If we efface the four edges PR and make aPc covertical over ePg, c and e on the left, and bRd covertical over hRf, b and h on the left, cPg crossing aPe, and bRf crossing dRh, we have before us the two summits P and R of M_n. Draw this figure, and call it (C). No change has been made at any crossing besides P and R. Beginning to read in (C) along cPg, we find the two circles



$$cPg\ldots hRd\ldots,\ \text{and}\ aPe\ldots fRb\ldots,$$

of the bifilar M_n; for Pg in (B) brings us to hR, *i.e.*, in (C) to hRd; and Rd in (A) brings us to cP, *i.e.*, in (C) to cPg, repeating the round. Also along aPe in (C) we reach by (B) fR, *i.e.*, in (C) fRb; and Rb in (A) brings us to aP, *i.e.*, in (C) to aPe, repeating the circle.

3. Let us now unkiss at R in (C), so as to make the section PR into Prr'. We are to read not fb and hd crossing at R, but fd and hb kissing at R; and evidently this can make no difference in the course of the thread from R through b, d, h, or f. We find the circle of an unifilar of $n-1$ crossings,

$$frd\ldots cPg\ldots hr'b\ldots aPe\ldots,$$

or omitting the creases r and r',

$$fd\ldots cPg\ldots hb\ldots aPe\ldots,$$

containing all the sequences $d\ldots c$, $g\ldots h$, $b\ldots a$, $e\ldots f$; for Rd in (A) brings us to cP, *i.e.*, by (C) to cPg; and Pg in (B) leads to hR, *i.e.*, in C to $hr'b$, whence Rb in (A) brings us to aP, *i.e.*, by (C) to aPe; and Pe in (B) brings us to our start in frd.

Unkissing next at P we get the circle

$$ga\ldots bRf\ldots ec\ldots dRh\ldots,$$

of an unifilar on which lies the triangular section Rpp'. Thus we have proved

Theorem E.—If we unite by the section ffc two bifilar knots K_{e+2} and L_{n-e} by bifilar 2-gons PR and PR, we construct a bifilar M_n, which gives by unkissing at P and R two unifilar knots each of $n-1$ crossings.

4. (*b*) *Bifilar on Even of Unifilar.*—Let the pair

$\ldots a\mathrm{PR}b \ldots$ and $c\mathrm{PR}d \ldots$ (A)

be the circles of a bifilar knot K_{e+2}, having the bifilar 2-gon PR, and let

$\ldots e\mathrm{PR}f \ldots h\mathrm{RP}g \ldots$ (B)

be the circle of an unifilar L_{n-e} which has the even 2-gon PR; and let the knots be joined by the section *ffc*. We have a knot M_n whose crossings P and R show $c\mathrm{P}a$ covertical over $e\mathrm{P}g$, c and e on the left; and $b\mathrm{R}d$ covertical over $h\mathrm{R}f$, b and h on the left. Call this figure (C). Reading from c in (C) we get the circle

$$..\, c\mathrm{P}g \ldots e\mathrm{P}a \ldots b\mathrm{R}f \ldots h\mathrm{R}d \ldots,$$

i.e., M_n is unifilar. Unkissing in (C) at R, we find the circle

$$..\, hb \,..\, a\mathrm{P}e \,..\, g\mathrm{P}c \ldots df \ldots$$

of an unifilar of $n-1$ crossings; and another such unifilar by unkissing at P, thus proving

Theorem F.—If the bifilar knot K_{e+2}, at its bifilar 2-gon PR, be joined by section *ffc* to the unifilar L_{n-e} at its even 2-gon PR, an unifilar M_n is contracted which gives, by unkissing at P and at R, two unifilars of $n-1$ crossings.

5. (*c*) *Odd on Even of Unifilars.*—Let the circles

$..\, m\mathrm{PR}k \ldots s\mathrm{PR}i \ldots$ (A)
$\ldots f\mathrm{RP}e \ldots a\mathrm{PR}h \ldots$ (B)

be those containing the odd and even 2-gons PR, PR. Joined by section *ffc* they form the knot M_n, whose crossings P and R show $e\mathrm{P}a$ covertical over $s\mathrm{P}m$, e and s on the left, and $h\mathrm{R}f$ covertical over $k\mathrm{R}i$, h and k on the left. Call this configuration (C). Reading from e in (C), we get the circle

$$..\, e\mathrm{P}m \ldots i\mathrm{R}h \ldots f\mathrm{R}r \ldots s\mathrm{P}a \,..,$$

proving M_n unifilar.

Unkissing at R in (C) we get

$$if \ldots hk \ldots s\mathrm{P}a \ldots e\mathrm{P}m \ldots$$

the circle of an unifilar of $n-1$ crossings; and by unkissing at P we find another such unifilar, thus establishing

Theorem G.—If at its odd 2-gon PR, we join by section *ffc* the unifilar K_{e+2} and the unifilar L_{n-e} at its even 2-gon PR, we construct an unifilar M_n, from which by unkissing at P and R we obtain two unifilars each of $n-1$ crossings.

6. (*d*) *Odd on Odd of Unifilars.*—Let

$$\dots a\mathrm{PR}h \dots e\mathrm{PR}f \dots \qquad (\mathrm{A})$$
$$\dots m\mathrm{PR}k \dots s\mathrm{PR}i \dots \qquad (\mathrm{B})$$

be the circles of the unifilars K_{e+2} and L_{n-e} which have each an odd 2-gon PR. Making the angle $e\mathrm{P}a$ covertical over $s\mathrm{P}m$, e and s on the left, and $h\mathrm{R}f$ over $k\mathrm{R}i$, h and k on the left, we have the crossings P and R of M_n, the result of joining (A) and (B) by the section *ffc*. Call this figure (C). Reading in (C) from e, we get the two circles

$$\dots e\mathrm{P}m \dots i\mathrm{R}h \dots \text{ and } \dots a\mathrm{P}s \dots k\mathrm{R}f \dots,$$

proving that M_n is bifilar.

Unkissing at R in (C) we obtain

$$\mathrm{K}h \dots e\mathrm{P}m \dots if \dots a\mathrm{P}s \dots$$

an unifilar of $n-1$ crossings, which has every summit of (A) and (B) except R, and every edge of them but the four PR's. And by unkissing at P in (C) we get another such unifilar. This proves

Theorem H.—If at their odd 2-gons PR, PR, we unite the unifilars K_{e+2} and L_{n-e} by the section *ffc*, we construct a bifilar M_n, which yields by unkissing at P and at R, two unifilars each of $n-1$ crossings.

7. (*e*) *Even on Even of Unifilars.*—Let

$$\dots a\mathrm{PR}h \dots f\mathrm{RP}e \dots \qquad (\mathrm{A})$$
$$\dots s\mathrm{PR}i \dots k\mathrm{RP}m \dots \qquad (\mathrm{B})$$

be the circles of two unifilars having each an even 2-gon PR. Uniting them at those 2-gons by the section *ffc*, we get the knot M_n whose R linear section PR shows at P the angle $e\mathrm{P}a$ over $s\mathrm{P}m$, e and s on the left, and at R the angle $h\mathrm{R}f$ over $i\mathrm{R}k$, h and i on the left. Call this figure (C). Reading from e and h in (C) we get two circles

$$\ldots ePm \ldots sPa \ldots, \text{ and } hRh \ldots iRf \ldots,$$

of the bifilar M_n.

Unkissing at R we get the circle

$$\ldots hi \ldots kf \ldots$$

which contains neither of the sequences $e \ldots a$ and $m \ldots s$, so that its knot of $n - 1$ crossings is no unifilar.

Unkissing at P, we get the circle

$$\ldots me \ldots as \ldots$$

which contains neither $i \ldots k$, nor $f \ldots h$, and is not the circle of an unifilar. Both the knots of $n - 1$ crossings are plurifils. This proves

Theorem J.—If at their even 2-gons PR, PR, we unite two unifilars K_{e+2}, and L_{n-e} by the section ffc, we construct a bifilar knot M_n, which gives by unkissing at P and R two plurifil knots each of $n - 1$ crossings.

8. (*f*) *Bifilar on Odd of Unifilar.*—Let

$$\ldots aPRb \ldots \text{ and } \ldots cPRd \ldots \qquad \text{(A)}$$

be the circles of a bifilar knot having the bifilar 2-gon PR. And let

$$\ldots ePRf \ldots hPRg \ldots \qquad \text{(B)}$$

be the circle of an unifilar, on which is the odd 2-gon PR. Drawing the angle aPc covertical over hPe, a and h on the left, and bRd covertical over gRf, b and g on the left, we have the crossings P and R of M_n constructed by the union of the knots at their 2-gons PR. Call this figure (C). Reading from a in (C) we find the circle

$$\ldots aPe \ldots gRd \ldots cPh \ldots fRb \ldots$$

of this M_n, showing that it is unifilar. Unkiss now at R in (C); we get

$$\ldots Fd \ldots cPh \ldots$$

a circle of the knot so formed of $n - 1$ crossings, which contains neither $d \ldots a$, nor $g \ldots e$; so that the knot is not unifilar; and by unkissing at P, we form another plurifil knot of $n-1$ crossings. This proves

Theorem K.—If we unite the bifilar knot K_{e+2} at its bifilar 2-gon PR to the unifilar L_{n-e} by its odd 2-gon PR, we construct an unifilar M_n of n crossings, which yields by unkissing at P and at R two plurifil knots each of $n-1$ crossings.

9. It is evident from the above reasoning that in the construction of M_n which has a linear section PR, at which by unkissing two unifilars of $n-1$ crossings can be got, we are in every case to use one of the Theorems E, F, G, H, in articles 3, 4, 5, 6; and that these are both necessary and sufficient rules. By them we can form all the required unsolid knots M_n without omission or repetition.

We have only to lay e upon $n-e$, *i.e.* K_{e+2} upon L_{n-e}, where $e+2 \gtrdot n-e$, beginning at $e=2$.

In forming the unsolid knots (M_{12}) which I herewith present to Professor Tait, I have begun with $e=4$; *i.e.*, I have laid 4 upon 8, and 5 upon 7. For not only is the number of figures for $e=2$ and $e=3$ enormous, but I am sure that they would be of no service to Professor Tait, in his rapid handling of his twists. Nor am I at all sanguine in the hope that these results for $e=4$ and $e=5$ will assist him except by furnishing a ready mode of verifying the most complex work of his grouping of the 11 folds with which I have had the honour to supply him. I have for myself never attempted this task of grouping the twisted knots of eight or more crossings, as this problem appears to me of less consequence than an accurate census of the knots with a description of their symmetry.

10. I should have remarked in my paper "On the Twists, &c.," that a complementary pair of n-fold unsolids will give only one couple of convertible $(n-1)$ folds, if each of the pair has P and R alike, which can be only when a zonal trace passes between P and R. And I was in error in saying that "every 9-fold so got from P and R alike is a 9-fold having a triangular section at which it can be twisted into its reflected image." The truth is that every $n-1$-fold got from P and R alike on the knot M_n, whether M_n has or has not a complementary, is a knot which has a triangular section at which it can be twisted either into itself or into its reflected image.

I believe that no M_{12} that I have drawn, which has no complementary about the section PR, has P and R alike; and that such drawn M_n will always give a pair of mutually convertibles.

11. Every K_{e+2} imposed on L_{n-e} in the (M_n) above constructed is supposed to be either a subsolid defined in page 283 of *Trans. R.S.E.*, vol. xxxii. part 2; or such a subsolid carrying a plural flap, of which neither P nor R is a crossing.

In laying 4 on 8, I have never laid 2·2 on 8; nor have I laid 2·3 nor 3·2 on 6 (*vide*, for this kind of operation, the memoir above cited); and this for the reason given in art. 9.

12. The number of unsolid knots (M_{12}), a portion of U_{12} (see my paper "On the Twists, &c."), which I herewith offer, is 577, of which 199 are bifilars and 378 unifilars. They are few in comparison of the entire U_{12}. As they will be useful, if ever the 12-fold knots are wanted, it is worth the while to give a summary of them, which will, to a student working on this subject, be of almost as much service as the 577 engraved figures. The references to ${}_7$A, &c.; ${}_8$A, &c., are to knots nearly all engraved in my plates in the *Trans. R.S.E.*, and clearly given in the text when not engraved.

Under the heading, *Bifilar on bifilar*, 4 on 8, are 47 bifilars, constructed on

${}_8$A 2,
${}_8$C 4,
${}_8$D 2,
${}_8$I 4,
${}_8$P 2,
${}_8$R 4,
${}_8$S 3,
${}_8$U 4,
${}_8$V 2,
${}_8$A*w* 6,
${}_8$B*a* 1,
${}_8$B*k* 2,
${}_8$B*l* 6,
${}_8$B*m* 2,
${}_8$B*x* 3, of
the 47.

Under *Even of unifilar on bifilar*, 4 on 8, are 67 unifilars, constructed on

${}_8$A 3,
${}_8$C 6,
${}_8$D 3,
${}_8$E 6,
${}_8$P 3,
${}_8$R 5,
${}_8$S 3,
${}_8$U 4,
${}_8$V 3,
${}_8$A*w* 9,
${}_8$B*a* 2,
${}_8$B*k* 3,
${}_8$B*l* 7,
${}_8$B*m* 2,
${}_8$B*x* 6, of
the 67.

Under *Bifilar on even of unifilar*, 4 on 8, are 47 unifilars, constructed on

${}_8$E 4,
${}_8$F 1,
${}_8$G 4,
${}_8$L 4,
${}_8$Q 1,
${}_8$Y 1,
${}_8$A*t* 4,
${}_8$A*u* 4,
${}_8$A*v* 4,
${}_8$A*x* 6,
${}_8$B*n* 4,
${}_8$B*v* 2,
${}_8$B*w* 5,
${}_8$B*z* 3.

Under *Odd on even of unifilars,* 4 on 8, are 88 unifilar knots, constructed on

$_8$E 8,
$_8$F 2,
$_8$G 8,
$_8$L 8,
$_8$Q 2,
$_8$Y 2,
$_8$A*t* 8,
$_8$A*u* 8,
$_8$A*v* 8,
$_8$A*x* 12,
$_8$B*n* 6,
$_8$B*q* 1,
$_8$B*v* 2,
$_8$B*w* 8,
$_8$B*z* 5.

Under *Even on odd of unifilars,* 4 on 8, are 81 unifilar knots, constructed on

$_8$B 3,
$_8$E 3,
$_8$F 3,
$_8$G 3,
$_8$Q 3,
$_8$T 3,
$_8$X 3,
$_8$A*b* 3,
$_8$A*u* 3,
$_8$A*w* 6,
$_8$A*x* 3,
$_8$A*z* 12,
$_8$B*i* 6,
$_8$B*j* 12,
$_8$B*n* 3,
$_8$B*q* 4,
$_8$B*v* 6,
$_8$B*w* 3.

Under *Odd on odd of unifilars,* 4 on 8, are 108 bifilar knots constructed on

$_8$B 4,
$_8$E 4,
$_8$F 4,
$_8$G 4,
$_8$Q 4,
$_8$T 4,
$_8$X 4,
$_8$A*t* 4,
$_8$A*u* 4,
$_8$A*w* 4,
$_8$A*x* 4,
$_8$A*z* 16,
$_8$B*i* 8,
$_8$B*j* 8,
$_8$B*n* 4,
$_8$B*q* 4,
$_8$B*v* 4,
$_8$B*w* 4.

Under *Bifilar on bifilar,* 5 on 7, in which of K_{5+2} and L_{12-5} either is laid on the other, are 36 bifilars, on

$_7$F$_7$F 4,
$_7$F$_7$K 4,
$_7$F$_7$M 2,
$_7$F$_7$N 2,
$_7$F$_7$R 2,
$_7$K$_7$K 4,
$_7$K$_7$M 2,
$_7$K$_7$N 2,
$_7$K$_7$R 2,
$_7$M$_7$M 2,
$_7$M$_7$N 2,
$_7$M$_7$R 2,
$_7$N$_7$N 2,
$_7$N$_7$R 2,
$_7$N$_7$R 2.

Under *Even of unifilar on bifilar,* 5 on 7, are 63 unifilars, constructed on

$_7$C$_7$F 4,
$_7$C$_7$K 4,
$_7$C$_7$M 2,
$_7$C$_7$N 2,
$_7$C$_7$R 2,
$_7$D$_7$F 4,
$_7$D$_7$K 4,
$_7$D$_7$M 2,
$_7$D$_7$N 2,
$_7$D$_7$R 2,
$_7$E$_7$F 8,
$_7$E$_7$K 8,
$_7$E$_7$M 4,
$_7$E$_7$N 4,
$_7$E$_7$R 4,
$_7$L$_7$F 2,
$_7$L$_7$K 2,
$_7$L$_7$M 1,
$_7$L$_7$N 1,
$_7$L$_7$R 1.

Under *Odd on even of unifilars,* 5 on 7, are 32 unifilars, constructed on

${}_7E_7C$ 4,
${}_7E_7D$ 4,
${}_7E_7E$ 8,
${}_7E_7L$ 2,
${}_7L_7C$ 2,
${}_7L_7D$ 2,
${}_7L_7E$ 4,
${}_7L_7L$ 1,
${}_7U_7C$ 1,
${}_7U_7D$ 1,
${}_7U_7E$ 2,
${}_7U_7L$ 1.

Under *Odd on odd of unifilars,* 5 on 7, are 8 bifilars, constructed on

${}_7E_7E$ 4,
${}_7E_7L$ 2,
${}_7E_7U$ 1,
${}_7L_7U$ 1.

Every one m_{12} of these 577 knots gives by unkissing at P and R of its linear section PR two unifilar 11-folds which, if the knot m_{12} has no complementary about PR, can be twisted either one into the other, and which, if m_{12} has a complementary, are two knots in a group of mutually convertibles, if I rightly conceive the matter. But not every two in a group can be twisted each into the other by a single twist. When m_{12} has a complementary, two pairs of unifilar 11-folds are thereby given, each pair being two knots convertible into each other by a single twist, and both pairs, if I am not in this point mistaken, belonging to the same group.

When m_{12} has a complementary about PR, it stands drawn next to m_{12} among the 577 figures herewith presented.

I have had no difficulty in the cases 1 and 2 of art. 1, because I have the bifilars of 8 and 9 crossings. As the bifilars of ten crossings are not before the student, he may find it useful to know how to lay 2 of the unifilar K_{2+2} on the bifilar L_{12-2}, without drawing the bifilar 10-folds, and by inspection only of the unifilar 9-folds. The rule is this:—Make every odd angle at the crossing P of an unifilar 9-fold covertical with the figure Pab, abR; *i.e.*, with two triangles collateral with the 2-gon ab and having their third angles at P and R in the constructed linear section PR. Every result of laying an even 2-gon on a bifilar 2-gon of a 10-fold is thus obtained. The proof of this is easy by my theorems A and C given recently in these *Proceedings*, applied to the figure Pab abR on the 12-fold completed by it. By similar devices the use of bifilar bases and charges can always be evaded in our constructions.

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Fig. I.

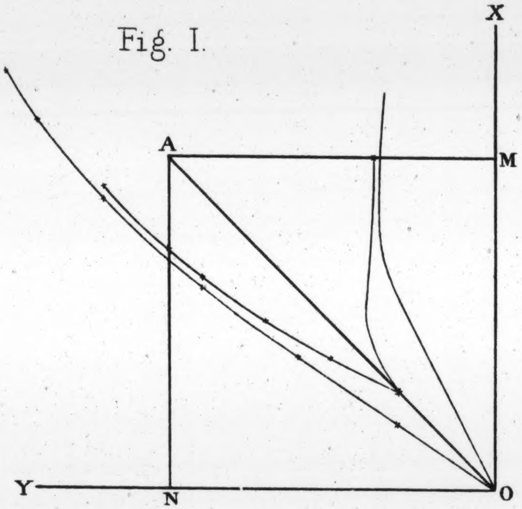


Fig. II.

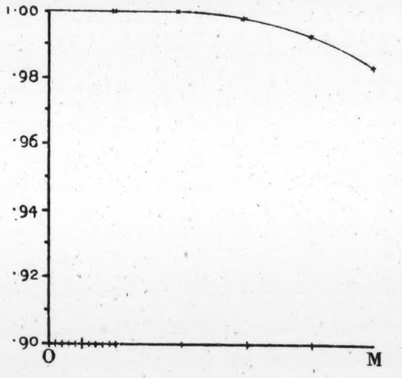


Fig. III.

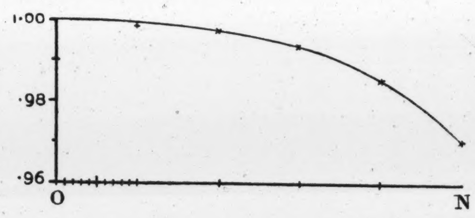


Fig. IV.

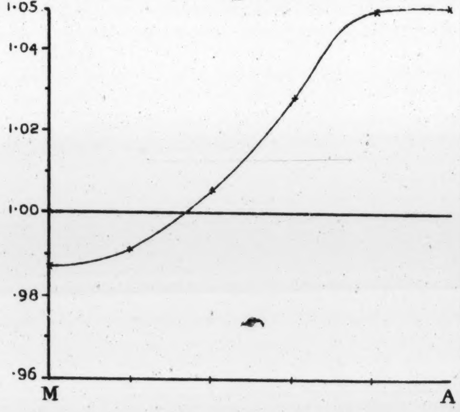


Fig. V.

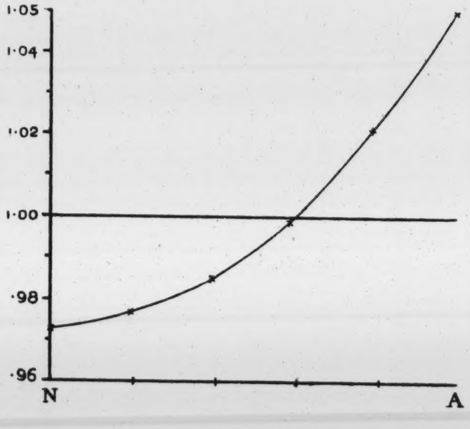
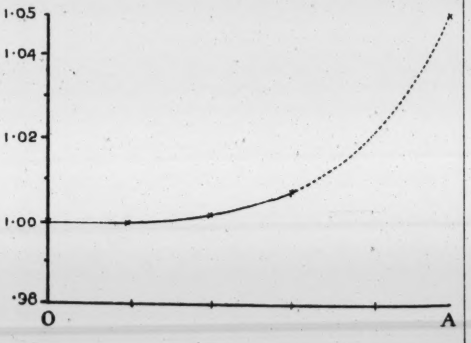


Fig. VI.



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5. Experiments on the Field of a Helmholtz Tangent Galvanometer. By R. Ellis, C.E.; R. D. Clarkson; and H. Rainy, M.A. (Plate XVIII.)

The object of the following experiments, which were suggested by Professor Chrystal, was to determine the relative values of the intensity of the horizontal component of magnetic force, due to a current flowing in the coils of the Helmholtz galvanometer, for different parts of its field.

The experiments were conducted as follows:—A sheet of ground glass was carefully ruled into squares, whose sides were 1 cm. long, and it was placed horizontally within the coils of the Helmholtz galvanometer. On it stood a sensitive reflecting galvanometer, the horizontal plane of the centre of the mirror containing, as nearly as possible, the axis of the coils of the Helmholtz galvanometer. The position of the sensitive galvanometer with reference to the centre of the Helmholtz field could be readily obtained by means of the graduated glass plate.

For the first experiment the sensitive galvanometer was placed in the centre of the field, when a current was split and the two parts sent in opposite directions through the coils of the sensitive galvanometer and of the Helmholtz respectively; resistances being added to the former circuit until no deflection of the needle took place. The resistance of the whole circuit of the sensitive galvanometer was then determined; after which the experiment was repeated to guard against any accidental error. The sensitive galvanometer was then removed to the next point of the field which was to be tested, and the same process repeated; when the intensity at that point (that at the centre being taken as unity) was obtained by dividing the former by the latter resistance.

The error in placing the sensitive galvanometer was within ± 1 mm. at any part of the field; the error in the graduation of the glass plate was less than 1 per cent. A very slight alteration of the resistance inserted in the circuit—less than ·0001 of the total—produced a very considerable deflection.

No perceptible error was introduced if the axes of the two instruments were slightly inclined to each other, instead of being exactly parallel. If the axes of the instruments made with each other an angle of $\tan^{-1} 1/6$ [*i.e.*, $\tan^{-1} 1{\cdot}\dot{6} = 9^\circ\ 28'$], scarcely any difference was perceptible in the readings from those taken with the axes parallel; a marked difference is visible if the angle be $\tan^{-1} 1/3$ [*i.e.*, $\tan^{-1} {\cdot}\dot{3} = 18^\circ\ 26'$].

Experiments repeated with a small galvanometer on the Helmholtz model instead of the sensitive galvanometer, proved that no great error had been introduced by any accidental change of relative position between the coils and needle of the reflecting galvanometer, such as might have been produced by a slight tilt to the side.

The dimensions, &c., of the coils of the Helmholtz galvanometer are as follows :—

Each coil consists of 16 turns of wire, and is of rectangular section. The wire is wound on in 4 layers, each of 4 turns. The mean radius of both coils is 24·65 ± ·02 cm., the distance between their centres is 24·51 cms. The following are the measurements for each layer :—

Bed of coils, . . .	24·18 cm.
Outer side of 1st layer, . .	24·41 cm.
„ 2nd „ . .	24·66 cm.
„ 3rd „ . .	24·89 cm.
„ 4th „ . .	25·12 cm.

Explanation of the Tables.

The tables are arranged as follows :—In the first column is a reference number to the experiment; in the second column the position of the centre of the needle is recorded; y being the direction along the Helmholtz axis, and x the direction perpendicular to y in the horizontal plane; the numbers being the distances in cms. from the origin. The third column contains the values of the resistance ξ of the reflecting galvanometer circuit; the fourth the logarithms of these resistances; the fifth the resulting value of the intensity $\mathfrak{J}$ of that point of the field, obtained by the formula $\mathfrak{J} = \frac{\xi'}{\xi}$, where ξ' is the value of ξ for the centre of the field; while the sixth shows the difference of each value of $\mathfrak{H}$ from that preceding it, except in the case of Exp. VII., where the difference is between the value of $\mathfrak{H}$ for its position, and that at the centre.

There is also a table giving two series of pairs of points, the value of $\mathfrak{H}$ being constant throughout each series. Thus there are two points in every quadrant, for *each* value of x, which give the same value of $\mathfrak{H}$.

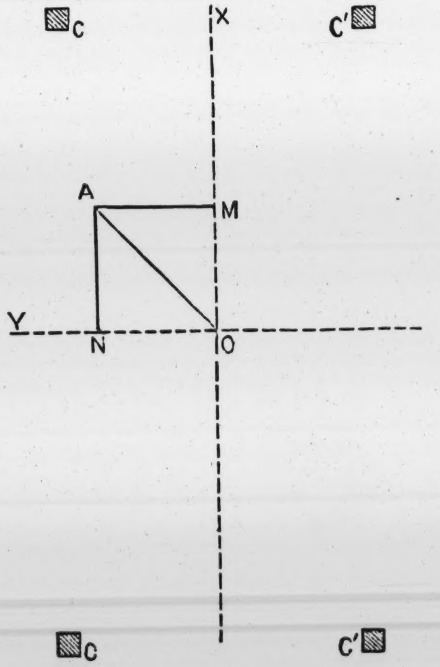
Explanation of the Diagrams (Plate XVIII.).

Fig. 1 represents a portion of the Helmholtz field, where O is the centre, and OY the axis. The two pairs of curved lines are of equal values of $\mathfrak{H}$, being constructed from the data given in the last table. The scale in this figure is 5 divisions per cm.

Figs. 2, 3, 4, and 5 represent the values of $\mathfrak{H}$ along OM, ON, MA, and NA respectively; the scale for the distance along these lines being in every case 5 small divisions per cm. along the one co-ordinate, while the values of $\mathfrak{H}$ are measured along the other.

In fig. 6 the values of $\mathfrak{H}$ along OA are represented; but, as it is inconvenient to represent the actual distances of the points along the diagonal on same scale, the distances from the centre of their projections on the axis are substituted.

The accompanying sketch represents the whole of the horizontal section along the axis of the Helmholtz, indicating the position of the part of the field given in fig. 1, CC and C′C′ being the two coils, and OY the axis of the galvonometer.



Number of Expert.	Position.		ξ	Log ξ.	$\mathfrak{J}$	Differences.
	x,	y.				
I.	0,	0	68·360	1·8348021	1·000000	·000000
II.	2,	0	68·360	1·8348021	1·000000	·000000
III.	4,	0	68·360	1·8348021	1·000000	·000000
IV.	6,	0	68·523	1·8358364	·997621	− ·002379
V.	8,	0	68·840	1·8378409	·993027	− ·004594
VI.	10,	0	69·380	1·8412343	·985298	− ·007729
VII.	0,	2	68·506	1·8357286	·997869	− ·002131
VIII.	0,	4	68·563	1·8360898	·997039	− ·000830
IX.	0,	6	68·810	1·8376516	·993460	− ·003579
X.	0,	8	69·360	1·8411091	·985582	− ·007878
XI.	0,	10	70·380	1·8474493	·971298	− ·014284
I.	0,	0	68·880	1·8380931	1·000000	
XII.	2,	2	68·880	1·8380931	1·000000	·000000
XIII.	4,	4	68·710	1·8370199	1·002473	+ ·002473
XIV.	6,	6	68·360	1·8348021	1·007607	+ ·005134
I.	0,	0	68·140	1·8334021	1·000000	
XV.	0,	10	69·920	1·8446014	·974542	[− ·025458]
XVI.	2,	10	69·730	1·8434197	·977063	+ ·002521
XVII.	4,	10	69·175	1·8399492	·985038	+ ·007975
XVIII.	6,	10	68·180	1·8336570	·999413	+ ·014375
XIX.	8,	10	66·720	1·8242560	1·021283	+ ·021870
XX.	10,	10	64·860	1·8119769	1·050570	+ ·029287
I.*	0,	0	68·0927	1·83310049	1·000000	
XXI.	10,	0	68·957	1·8385784	·987466	[− ·012534]
XXII.	10,	2	68·690	1·8368935	·991304	+ ·003838
XXIII.	10,	4	67·483	1·8291944	1·009035	+ ·017731
XXIV.	10,	6	66·390	1·8221027	1·025646	+ ·016611
XXV.	10,	8	64·844	1·8118698	1·050100	+ ·024454
XXVI.	10,	10	64·815	1·8116755	1·050570	+ ·000470

* Calculated value.

The following positions required no alteration in the value of ξ:—

Set I.

x.	y.	
0·0	0·0	
2·0	1·0	3·0
3·3	...	4·0
4·0	...	6·0
5·6	...	8·0
6·0	3·0	9·0
7·2	...	10·0
8·7	...	12·0
10·0	3·5	...
11·1	...	14·0
12·0	3·4	...
12·6	...	15·0

Set. II.

x.	y.	
3·0	3·0	
5·0	4·0	7
6·4	...	9·0
7·0	3·9	...
7·3	...	10·0
9·1	...	12·0
10·0	3·6	...

6. On the Partition of Energy between Systems of Colliding Spheres. By Professor Tait.

7. On the Effect of Pressure on the Maximum Density Point of Water. By Professor Tait.

8. Chemical and Microscopical Studies on the Action of Salicylic Acid on Ferments. By Dr A. B. Griffiths, F.R.S.E., F.C.S. (Lond. and Paris), Lecturer on Chemistry, Technical College, Manchester, &c.

My attention has been occupied of late with a chemical and microscopical study on the action of various compounds on the lower forms of plant-life. I have already presented to the Chemical Society of London a paper on the Action of a Solution of Ferrous Sulphate on certain Parasitic Diseases which attack our Crops. (This paper will be published presently.) I wish here to detail some studies on the action of salicylic acid ($C_7H_6O_3$) on certain ferments (organised and unorganised). A solution of salicylic acid was prepared containing 0·2 gram. of the acid in 1000 c.c. of water.

A drop of yeast was placed upon a slide under the microscope, and then a few drops of the above salicylic acid solution was run in between the slide and cover-slip, when I found that it had no action on the true alcoholic ferment, for I was *unable* to stain the protoplasm of the cells with a solution of eosin, showing that the protoplasm was not dead. But, when I operated in a like manner on certain other organised ferments, namely, *Mycoderma aceti*, *Bacterium lactis,* and the *Butyric bacillus,* it was very different. On running in the same salicylic acid solution upon slides containing these various ferments they were quickly destroyed. What was observed under the highest powers of the microscope was that the salicylic acid acts chemically upon the cellulose, or the form of cellulose making the external walls of these lowly organisms. It was evident that the acid dissolved the cellulose wall, and in some cases I could see that the cellulose wall had been perforated by the acid solution.

Beyond these facts, I have found that the above named aqueous solution of salicylic acid also prevents the chemical action of hydration by means of the soluble zymases. If yeast is added to a solution of cane sugar, and to this solution the salicylic acid solution is added, *no* decomposition according to the following equation takes place:—

$$C_{12}H_{22}O_{11} + H_2O = C_6H_{12}O_6 + C_6H_{12}O_6 .$$

That is, the salicylic acid has acted upon the soluble zymase which is secreted by the *Torula cerevisiæ.* Hence no fermentation takes place. I allowed the above to stand for two or three days at a temperature most suitable to engender alcoholic fermentation (about 80° F.), and then tested for glucose sugars by means of Fehling's solution without any result.

I also tried the action of salicylic acid solution upon a solution of starch which had previously been inoculated with a small quantity of saliva. After standing several days, I could not find the smallest trace of glucose sugar.

From this, salicylic acid acts upon the soluble ferment (ptyalin) contained in saliva, preventing the hydrating action upon the amyloses.

Then again, I have found that the above solution of salicylic acid acts chemically upon the cellulose walls of *dead* Torulæ, destroying them in a similar manner to the organised ferments already described at the commencement of this paper, but it has no action on the *living* Torula. This shows that a chemical change must have taken place in the molecular structure of the cellulose wall of the cell after the death of the organism.

It is a well-known fact, that in every brewery the yeast becomes deteriorated at certain times, and hence the beer brewed by such yeast is not so good as formerly. This is due to "diseased ferments" in the yeast (viz., the organisms I have alluded to in the early part of this paper). The common remedy is for the brewer to change his yeast. But I have found that this is not essential, if the brewer waters the "diseased yeast" (*i.e.*, yeast containing these thread-like ferments) with a solution of salicylic acid, the "disease ferments" are all destroyed, and the yeast is not acted upon by this solution. Yet at the same time the yeast so treated is not so active in its decomposition of a glucose solution into alcohol. This yeast can be revivified by an aqueous solution containing 0·25 gram. of potassium nitrate and 0·2 gram. of sodium phosphate in 2000 c.c. of water. In fact, the Torula appears to decompose a much larger quantity of sugar (in wort) into alcohol in a given time after the above salts have been added to 2 titres of wort than when the wort is not so treated. It appears that the Torula lives its life-history to a certain extent by extracting the potash and phosphoric acid from the compounds containing them, which come into the wort from the barley and hops. Mitscherlich long ago showed that the ash of yeast gave no less than 53 to 59 per cent. of phosphoric acid, and from 28 to 39 per cent. of potash. Hence it may be that a larger amount of alcohol would be produced in beers by the addition of small quantities of the above substances to the wort.

From the above investigations the following conclusions are to be drawn :—

1. That a certain solution of salicylic acid has *no* action upon the *living* Torula, but dissolves it when dead; thus showing that some chemical change (post-mortem) has taken place in the cellulose of the cell-wall.

2. That the solution of salicylic acid destroys "disease ferments," by acting upon the cell-walls; showing that their cellulose most probably differs from the cellulose of the *Torula cerevisiæ*.

3. That the solution of salicylic acid prevents the hydrating action of the various soluble zymases.

4. A solution of sodium phosphate and potassium nitrate revivifies exhausted yeast, and even increases the yield of alcohol in saccharine solutions.

5. Salicylic acid acts as an antiseptic agent of great value, because it acts directly upon the "disease ferments" in beers, and not upon the true alcoholic ferment.

6. Salicylic acid is not a poison in quantities far exceeding the amount in the solution given in this paper. This acid is largely used in France and Germany as a medicine.

For those interested in the application of this acid in medicine, I refer them to the following excellent memoirs :—

(*a*) Wagner's "Le traitement de la diphterie des maladies de l'estomac et des intestins," *Moniteur Scientifique*, 1885, p. 354; also *Journal für praktische Chemie*, xi. pp. 57 and 211.

(*b*) Dr Germain See's "L'acide salicylique et le salicylates dans le traitement de la goutte et des rhumatismes," *Rapport à l'académie de Médecine de Paris*, June and July 1877.

(*c*) Fontheim's "De l'action de l'acide salicylique employé comme médicament," *Moniteur Scientifique*, 1875, p. 853.

PRIVATE BUSINESS.

Letters were read by the Secretary from Mr Dudgeon of Cargen, and from Mr Aitken, on the recent display of Iridescent Clouds.

Mr A. J. G. Barclay was balloted for, and declared duly elected a Fellow of the Society.

Monday, 18th January 1886.

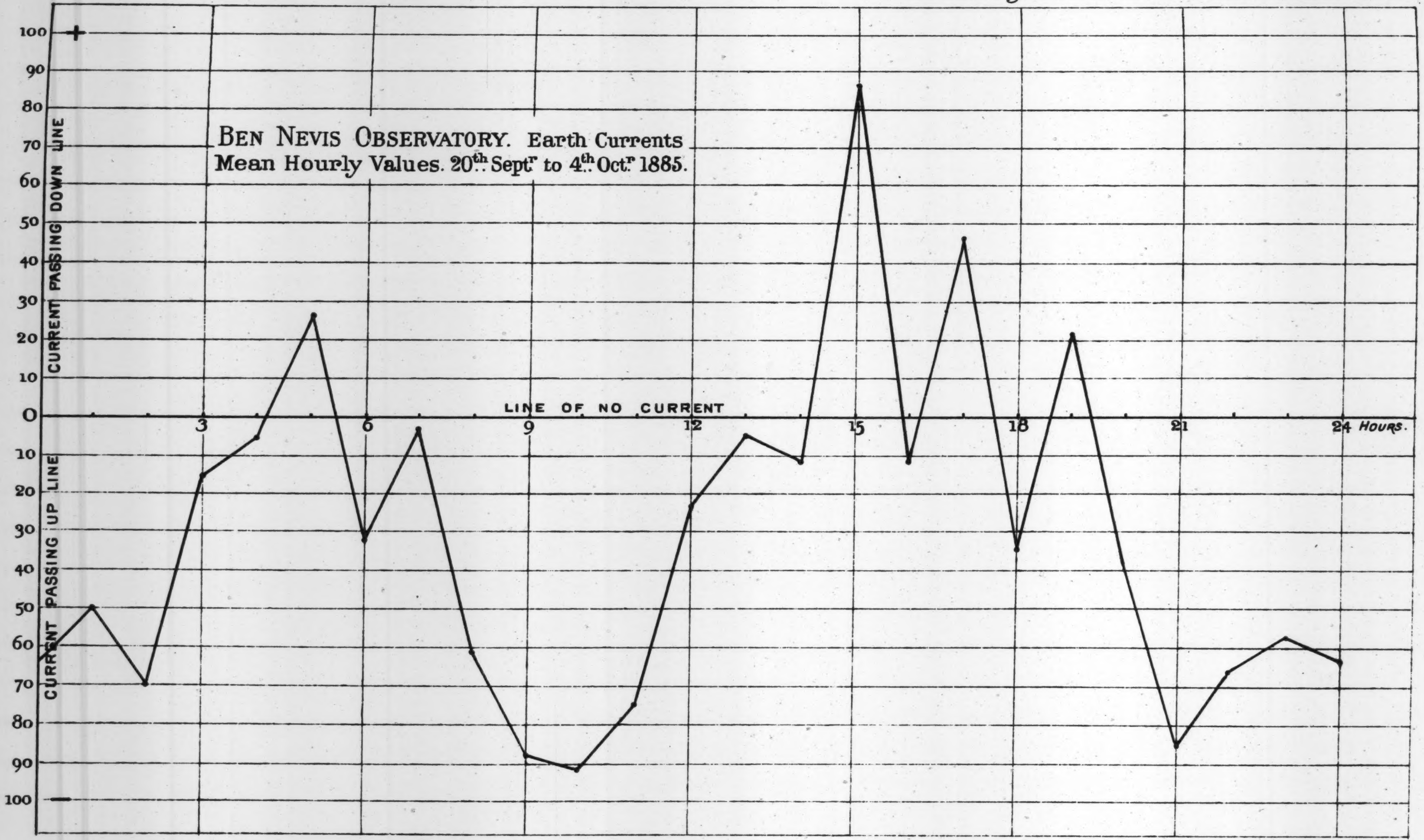
SHERIFF FORBES IRVINE, Vice-President,
in the Chair.

The following Communications were read:—

1. On the Relation between the Densities and the Atomic Weights of the Elements. By Professor Crum Brown.

2. Observations on Earth-Currents in Ben Nevis Observatory Telegraph Cable. By H. N. Dickson. (Plate XIX.).

Telegraphic communication between Ben Nevis Observatory and Fort-William Post Office is kept up by means of a line which consists of an ordinary telegraph wire from Fort-William to Auchintee Farm at the base of the mountain, and a cable laid underground from thence to the summit. At Fort-William the wire is put to earth in the usual way, while at the Observatory the return wire is attached to the iron-wire sheath of the cable. Formerly the wire was worked by means of Wheatstone's ABC instruments, but in April last these were replaced by single needle "sounders." With both instruments the presence of earth-currents has been frequently observed, occasionally of such strength as to interfere with the proper transmission of signals, and from the working of the needle



BEN NEVIS OBSERVATORY. Earth Currents
Mean Hourly Values. 20th Sept^{r} to 4th Oct^{r} 1885.

M^{c}Farlane & Erskine, Lith^{rs} Edin^{r}

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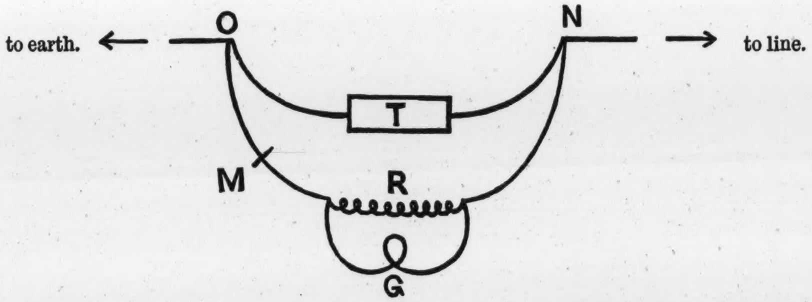
instrument especially, I was led to suppose that these currents were always present, and that they were subject to more or less regular variations, both in direction and intensity.

A detailed investigation would require more elaborate electrical apparatus than can at present be obtained at the Observatory, and would interfere with the ordinary use of the wire. The observations which form the subject of this communication were made entirely by means of a Thomson mirror galvanometer, and were so arranged as not to affect in any way the transmission of signals in either direction.

A preliminary trial was first made by putting a coil of copper wire in circuit, and placing in the field an ordinary pocket-compass. When this was done no observable deflection could be obtained. The mirror galvanometer was then put in, and after a few further trials the arrangement shown below was adopted, and found to give suitable deflections.



In the figure O is binding screw leading to wire attached to sheath of cable.

N, binding screw attached to line wire of cable.

T, telegraph instrument.

A coil R, consisting of 24 feet of iron wire, No. 17 B.W.G., was attached to O and N, along with the telegraphic instrument, and in multiple arc with this the galvanometer G was inserted. Contact was broken at M, except while readings of G were actually being taken, so that no part of the current was diverted from the telegraph instrument. As no means of accurately measuring resistance was at hand, absolute measurements of the current passing at any time could not be obtained, but a rough estimate from tables of resistances of iron wire gave R = about 0·4 ohms; and the resistance of the telegraph instrument is not less than 200 ohms. The resistance of the galvanometer was given at 22·9 ohms, so that about $\frac{1}{10}$ of the whole current may be taken as the fraction passing through G. The galvanometer was powerfully controlled by external magnets, the time of vibration of the mirror being extremely short. The direction of the currents was determined directly by comparing the deflections due to them with those given by a Daniell cell.

We can by means of this arrangement ascertain at any time the direction of the currents passing through the line, and observe the variations in intensity from time to time. These currents I have assumed to be due to a difference of potential between the earth at Fort-William and the summit, and I think we are justified in treating the sheath of the cable as equivalent to an earth-plate at the Observatory, but it would be advisable in further investigations to have an actual earth-plate similar to that at the other end.

The observations extend from 20th September to 4th October 1885, and are hourly, the readings being taken just before the usual hourly meteorological observations. The curve of hourly values obtained by taking the mean deflection for each hour during the successive days shows a well-marked daily variation. During the early part of the night we have a current passing through the cable *up* the hill, which appears to reach a maximum at 21 hours, and again at 2 hours. After 2 hours a rapid diminution takes place, and at 5 hours we have the current reversed. Immediately after this the potential at the summit again falls relatively to that at Fort-William, and we have a current coming up, increasing in strength to a well-marked maximum at 10 hours. This is succeeded by another equalisation, and the current is again reversed about 13 hours, after which it passes *down* the line till about 18 hours. This part of the curve, however, is much more irregular, the current being very unsteady. After 18 hours the current again sets in an upward direction through the line, increasing pretty regularly till the maximum at 21 hours.

During the time over which these observations extend, the summit of Ben Nevis was almost continuously enveloped in fog or mist, and a succession of heavy storms passed over the Observatory about the end of September. The daily curve shown may therefore be taken to hold only for conditions under which the air is saturated, and further observations must be made before the actual daily variation can be determined. A careful comparison of the observations with the ordinary meteorological sheets of the Observatory, however, brings out some points of very great interest, especially when looked at in the light of the theory advanced by Professor C. Michie Smith, in a paper communicated by him to the Society on 1st June 1885 (*Trans. R.S.E.*, xxxii. p. 583). As the result of observations on atmospheric electricity, made on the summit of Dodabetta, the highest hill in the Neilgherries, Professor Smith finds that on the edge of a *dissolving* mist the potential is lower than the normal, while it is higher on the edge of a condensing mist. A study of the following table, with the notes appended, shows at once that in nearly every case where the top of Ben Nevis became clear for a short time, a strong current was found coming *up* the line, while as soon as the summit was again enveloped, the current was reversed. The connection between the moisture of the air and the earth currents is still further shown by studying the rainfall. During a fall of rain or snow the current is almost always found to pass *down* the cable; and, in the case of a sudden shower, the current sometimes drove the mirror of the galvanometer violently off the scale. A cessation of rain or snow will be seen in most cases to have exactly the opposite effect.

If we assume that the summit of Ben Nevis takes the potential of the masses of vapour covering it, and consider the earth-plate at the base as the real "earth" or zero of potential, it is at once obvious that these results agree with those of Professor Smith, and form important additional evidence of the truth of his theory, a conclusive proof of which would be of the greatest value in investigations connected with thunderstorms.

Following out the above conclusions, we should expect the mean daily curve for earth currents to be intimately connected with that for relative humidity, but as yet no observations have been taken except in saturated air. Further study of the curves shows that the currents are probably affected by other causes besides those connected with cloud. The approach of cyclones seems to disturb the electrical conditions to a certain extent, but the observations are not sufficiently numerous to allow of any definite inferences being drawn. My object in writing this paper has been chiefly to draw attention to the great additions which a complete series of electrical observations, made at Ben Nevis Observatory, would be likely to make to our knowledge of these subjects, if the experimental difficulties in the way could be surmounted.

The figures given represent the deflection of the spot of light of the galvanometer, in scale-divisions.

The sign + prefixed to a figure indicates that the current passed through the galvanometer *down* the line-wire to Fort-William. The opposite direction is indicated by a – sign. The sign ± indicates that the current was variable in direction. ±0 indicates that the current was extremely faint, the spot of light oscillating about the zero-point. The sign ∞ indicates that the spot of light was driven off the scale altogether. Where the current was very unsteady, the figures are in old style type.

September 20.—Ridge of high pressure between two cyclones, one lying to south-west and one to north-east. Centre of former advancing towards Denmark in afternoon.

21.—Decided change of wind from north-easterly to westerly at 12 hours. Barometer rising all day, with temporary dip, accompanied by southerly wind, from 18 to 20 hours.

22.—Depression with centre well out to north-west at 8 hours, passed to northwards in afternoon. Anticyclone to south-east. Rain in morning, diminishing from 11 hours to 13 hours, when wind shifted slightly; heavy rain at 17 hours. Wind backing till 18 hours, when it blew from south by west. Barometer fell till 14 hours, steady till 17 hours, after which fall continued till 22 hours. Temperature rising at night.

23.—Cyclone, centre to north-west going eastwards. Anticyclone to south-east, moving over Italy. Wind shifted more to north at 4 hours, and rain stopped, with temperature falling rapidly. Shower between 8 hours and 9 hours. Mist clearing at 10 hours—on again after 13 hours. A little snow, with shift of wind to easterly, at 16 hours. Wind changed to south-westerly, followed by snow, after 22 hours.

24.—Cyclone to north-east off coast of Norway—centre travelling eastwards along 70th parallel N. lat. Centre of anticyclone shifted to south-west of Ireland since 8 hours of previous day. Snow fell till 8 hours and after 11 hours. Currents and meteorological condi-

Table of Currents.

	Date.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24
September 1885, .	20	...	...	...	...	– 6	– 68	– 87	–128	– 62	–150	–110	– 28	+ 80	– 15	+109	+125	+ 39	+ 37	– 69	+ 22	– 88	– 49	– 92	– 38
,, ,, .	21	– 30	+ 29	– 49	+ 55	– 51	– 51	– 30	– 92	– 92	– 90	– 56	+ 18	+ 93	+ 80	+190	± 5	– 55	+ 83	+ 60	+ 29	–107	–107	– 33	– 5
,, ,, .	22	+ 58	– 53	– 20	± 87	+ 99	– 80	–126	–144	–190	–142	+ 45	+ 41	± 13	– 39	+276	–266	–304	–190	+ 74	– 19	–178	– 61	– 58	– 37
,, ,, .	23	– 89	–121	– 36	–245	+ 66	–263	–114	– 54	– 72	–315	+ 21	– 94	± 45	–111	+175	± 24	+190	–181	+126	– ∞	– 22	– ∞	+ 22	– 98
,, ,, .	24	+120	+ 47	+118	+113	+ 29	+ 30	+ 13	– 89	– 44	– 35	–125	– 8	– 13	– 6	+ 39	+ 46	+ 33	+ 21	+ 13	± 0	– 55	–109	–193	–124
,, ,, .	25	± 0	–229	+ 98	+ 25	+139	– 38	+168	– 50	–116	–100	–187	± 20	– 55	– 45	+ 77	+ 86	+243	– 16	+ 30	– 50	– 95	– 79	– 92	– 95
,, ,, .	26	–161	–146	– 20	+ 57	± 24	–102	+ 15	–114	– 97	–143	–168	...	–188	+ 6	+107	– ∞	– 58	+ 78	+ 38	– 25	– 31	– 90	–131	– 41
,, ,, .	27	– 14	– 39	– 4	– 8	+ 86	+ 85	– 35	–100	–240	+ 14	– 91	+ 14	– 48	+ 29	– 51	+164	+458	–311	– 80	– 73	– 72	+ ∞	– 30	–197
,, ,, .	28	– 91	– 40	+ 8	+ 17	+ 22	+ 30	+ 88	+ 7	– 74	– 52	– 97	– 40	+ 50	+ 24	+ 89	+103	+ 20	+ 39	+ 70	+129	– ∞	–296	– 31	–207
,, ,, .	29	– 77	– 28	– 15	– 20	+ 42	+ 44	+ 40	± 7	– 62	– 72	– 13	– 35	– 17	– 45	+105	+ 25	+ 9	– 18	+ 75	+ 12	– 7	– 86	– 67	–192
,, ,, .	30	–161	–113	– 56	+ 95	...	...	...	...	– 25	–144	–185	–117	+ 71	+ 29	+ 30	+ 51	+124	– 93	+ 19	+ 49	+ 9	– 34	– 13	– 48
October ,, .	1	– 65	– 71	– 34	–156	– 70	– 21	+ 59	+ 60	–123	–100	– 88	–102	– 10	– 11	+ 17	+ 6	+ 85	+ 55	17	± 35	+ 14	+ 60	+ 40	+ 57
,, ,, .	2	–230	– 77	– 53	– 48	+ 62	– 71	– 44	–131	+ 13	+ 36	+ 35	+ 51	– 42	– 29	+ 44	– 32	± 0	+ 22	– 11	+ 5	+ 9	+ 10	– 13	+ 34
,, ,, .	3	+ 37	–112	– 35	+ 57	– 34	+ 6	+ 41	+ 55	– 65	– 51	– 42	– 5	– 21	– 95	+ 24	– 15	– 96	– 26	– 10	– 45	– 67	– 24	– 87	+ 62
,, ,, .	4	+ 8	– 22	– 24	– 23	– 3	– 46	– 39	– 72	– 73	– 16	– 64	– 22	+ 18	+ 57	+ 63	+ 40	+ 7	– 15	+ 1	–110	– 84	–133	– 76	– 15
		– 50	– 70	– 16	– 6	+ 26	– 32	– 4	– 61	– 88	– 91	– 75	– 23	– 5	– 11	+ 86	– 11	+ 46	– 34	+ 21	– 38	– 85	– 66	– 57	– 63

tions generally very steady from 6 hours to 12 hours. Wind north-east by east.

25.—Centre of cyclone further eastwards and of anticyclone further westwards. Snow in intermittent showers all day. Fog clearing off at 5 hours, on again at 7 hours; clear from noon till 15 hours and after 23 hours.

26.—Centre of anticyclone further north—off west coast of British Isles. Cyclone advancing southwards and shallower. Fog very thin, clearing occasionally till 3 hours. Top clear from 12 hours till 18 hours, then fog occasionally till 21 hours. Sky cloudless at 22 hours. Snow in forenoon, ceasing at 16 hours. Scud passing at 23 hours.

27.—Anticyclone over south-west of Ireland, and giving way. Cyclone also giving way. Tending towards more uniform distribution of pressure. Rapid rise of temperature after 7 hours. Fog after 14 hours, except at 17 hours, when top cleared. Snow began to fall at 22 hours.

28.—Cyclones lying to north-west of Scotland and over Gulf of Bothnia. Anticyclone over Bay of Biscay. Wind backed till 7 hours, with drizzling rain, changing to heavy rain at 12 hours and sleet after 19 hours, with snow at 22 hours, followed by drizzling rain. Sudden dip of temperature at 21 hours.

29.—Cyclones to north of Shetlands and off north coast of Spain. Very deep depression approaching at night. Snow at 7 hours, with top clear for a short time. Top clear again at midnight.

30.—Galvanometer unworkable from 5 hours to 9 hours, owing to shaking of house by wind. Centre of storm passed at 9 hours.

October 1.—Brush discharge on lightning conductor at 5 hours 5 minutes, and at 14 hours. Cyclone passing off in morning. Another depression approaching from westwards.

2.—Cyclone advancing from westwards. Wind backed till 17 hours; minimum barometer at 18 hours. Very heavy rainfall.

3.—Cyclone passing off in forenoon. Another approaching; barometer began to fall at midnight. Snow all day.

4.—Maximum temperature at 12 hours. Wind backed and increased till 13 hours, veering and diminishing afterwards. Barometer continued falling.

3. On the Partition of Energy among Groups of Colliding Spheres. By Prof. Tait.

(*Abstract.*)

The second, only, of two short papers which I communicated to the Society a month ago, on points connected with the Kinetic Theory of gases, took account of the greater probability of collision between two particles as their *relative* speed is larger. I propose now to calculate the effect of this consideration on the results of my first paper.

The question resolves itself into finding the mean value of the expression

$$\mathrm{Pu}^2 - \mathrm{Qv}^2 - (\mathrm{P} - \mathrm{Q})\mathrm{uv} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

of that paper, in terms of the mean square speeds in the two systems; because, only when this is zero, does kinetic equilibrium set in.

We are here concerned only with collisions between particles belonging one to each system.

In my *Note on the Mean Free Path* (*anté*, p. 397) it is shown that the fraction of a group of particles, having speed v perpendicular to a layer of thickness δx, which collide, at angles from β to $\beta + \delta\beta$, with particles in that layer whose speed is v_1, is proportional to

$$\frac{\delta x}{v}\sqrt{v^2 + v_1^2 - 2vv_1\cos\beta}\,.\sin\beta d\beta\,.$$

Hence, in a given time, the number of such collisions is proportional to

$$\sqrt{v^2 + v_1^2 - 2vv_1\cos\beta}\,.\sin\beta d\beta = \bar{v}\sin\beta d\beta, \text{ suppose.}$$

Let V, V_1 be the projections of v, v_1 on the unit sphere, C that of the line of centres at collision. Then $\mathrm{VV}_1 = \beta$. Let also $\mathrm{VC} = a$, $\mathrm{CVV}_1 = \phi$. Finally, let n, n_1, represent the proportionate numbers of particles in the separate systems which have speeds v to $v + \delta v$, v_1 to $v_1 + \delta v_1$, respectively. Thus

$$n = \frac{4}{\sqrt{\pi}\,.\,\alpha^3}\varepsilon^{-pv^2}v^2dv\,,$$

$$n_1 = \frac{4}{\sqrt{\pi}.\,\beta^3}\varepsilon^{-qv_1^2}v_1^2dv_1\,;$$

where, for simplicity, p is put for $1/\alpha^2$ and q for $1/\beta^2$ in the exponentials. Then, for a v and a v_1, colliding, the mean value of u^2, the square of the speed of the v in the line of centres, is

$$\frac{\int nn_1\bar{v}\sin\beta d\beta\,.\,v^2\cos^2\alpha\,.\,\sin\alpha d\alpha d\phi}{\int nn_1\bar{v}\sin\beta d\beta\sin\alpha d\alpha d\phi}.$$

Here the limits of α ought to be restricted to a hemisphere of which the direction of relative motion is the axis. But, by extending them to the whole sphere, we merely double each integral, while we avoid some complex relations. We may compensate for this by taking ϕ from 0 to π only. [The same remark applies to the other integrals of this kind with which we have presently to deal.] The above fraction then takes the value

$$\frac{\frac{1}{3}\int nn_1\bar{v}\sin\beta d\beta v^2}{\int nn_1\bar{v}\sin\beta d\beta.}$$

Since $$\bar{v}^2 = v^2 + v_1^2 - 2vv_1\cos\beta,$$
this becomes

$$\frac{\frac{1}{3}\int nn_1\bar{v}^2 d\bar{v}\,v/v_1}{\int nn_1\bar{v}^2 d\bar{v}/vv_1}.$$

In further integrating this, we must remember that the limits of $\bar{v}$ are $v-v_1$ and $v+v_1$, while $v>v_1$, but v_1-v and v_1+v while $v<v_1$. Thus the denominator (being symmetrical) consists of two parts, which differ from one another only by the interchange of p and q. The value of the numerator can be found from that of the denominator by differentiation with regard to p and change of sign. When this is done we have for the mean value of u^2

$$\frac{3p+4q}{6p(p+q)}$$

This shows how the first and, by parity of reasoning, the second terms of (1) are affected by the new consideration. We might have directly verified this for the second term by evaluating

$$\frac{\int nn_1\bar{v}\sin\beta d\beta\,.\,v_1^2(\cos\alpha\cos\beta+\sin\alpha\sin\beta\cos\phi)^2\,.\,\sin\alpha d\alpha d\phi}{\int nn_1\bar{v}\sin\beta d\beta\sin\alpha d\alpha d\phi},$$

[After integration as to a and to ϕ, the part in brackets becomes a multiple of $\cos^2\beta + \sin^2\beta$.]

The average value of the product of the velocity-components along the line of centres (uv of (1)) is

$$\frac{\int nn_1\bar{v}\sin\beta d\beta \, . \, vv_1\cos a(\cos a\cos\beta + \sin a\sin\beta\cos\phi) \, . \, \sin a da d\phi}{\int nn_1\bar{v}\sin\beta d\beta \sin a da d\phi},$$

which is easily by the foregoing process reduced to

$$-1/6(p+q).$$

Thus it appears that the average value of (1), written in the form

$$\mathrm{P}(\mathrm{u}^2 - \mathrm{uv}) - \mathrm{Q}(\mathrm{v}^2 - \mathrm{uv})$$

is, term for term,

$$2\mathrm{P}/3p - 2\mathrm{Q}/3q,$$

or, in the usual notation,

$$\frac{2}{3}\left(\mathrm{P}\alpha^2 - \mathrm{Q}\beta^2\right),$$

so that Maxwell's Theorem is proved.

[*March* 15, 1886.—There is an error here, due to a wrong assumption as to the probabilities of various positions of the line of centres. It does not vitiate the proof, as it introduces a mere numerical factor of the whole quantity. See p. 644.]

4. On Water-Bottles, with the Description of a new form of Slip Water-Bottle. By Hugh Robert Mill, D.Sc., Scottish Marine Station. (Plate XX.)

The various forms of apparatus employed for procuring samples of water from beneath the surface are either designed to take up as a sample a mixture of the water in a certain vertical range, or to secure their entire charge at one definite position. The former description of water-bottle has been used almost invariably in deep-sea work; the latter is necessary only in shallow water.

The most common type, on the principle of the force-pump, appears to have been originated by Hooke* two hundred and twenty years ago. A box provided with a valve above and one below, both opening upwards, is lowered by a weight; the water runs through it easily, keeping the valves open; when it is pulled up the pressure of the water closes the valves, and if the upward motion is uninterrupted keeps them shut. A specimen of water is thus secured from nearly the greatest depth to which the apparatus has been sent.

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* Described and figured, *Phil. Trans.*, ii. (1667), p. 442.

This form has been worked out in many modifications; arrangements have been added for locking the bottle when it is full, and also for closing it by other means than changing the direction of motion.

In Marcet's machine for obtaining bottom samples,* the upper and lower valves were connected together, and during the descent of the instrument they were kept open by a cord and weight, which, ceasing to act on touching the bottom, allowed a spring to close and lock the valves. There was thus no risk of the contents changing during the upward journey. In a modification for obtaining intermediate samples the valves (in this case cones fitted on a rigid rod) were held up by a catch which could be withdrawn by a lever actuated by a weight slipped down the sounding line. The valves then closed, and were locked by a catch.

Wille's† water-bottle and Sigsbee's ‡ water-cup are the most approved modern developments of Hooke's apparatus. Wille's apparatus raised 5 litres of water, Sigsbee's only 8 ounces. Both are locked by the action of the current in the ascent on small screw propellers, which form part of the instrument. Tennant before 1819 devised a water-bottle § which was closed by the action of the water on "a small fly-wheel during its ascent." The arrangement consisted of a large water-tight snuff-box, the lid of which was held open against a powerful spring by a small wedge that was pulled out by the revolving fly-wheel.

Sir Robert Christison's cistern thermometer || is a water-bottle on Hooke's principle, with unvalved holes below, and carefully adjusted ball and cone valves above.

The water-bottles¶ employed on the "Porcupine" and "Lightning" expeditions were valved instruments without a locking

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* Described and figured, *Phil. Trans.*, cix. (1819), p. 208, pl. xi.

† Described and figured, *Norske Nordhavs Expedition*, iv. pt. 2, p. 17; and Tornoe's *Chemi* (1880), p. 13.

‡ Described and figured in Sigsbee's *Deep-Sea Sounding and Dredging* (1880), p. 91.

§ Described, *Phil. Trans.*, cix. (1819), p. 209.

|| *Proc. Roy. Soc. Edin.*, vii. (1872), p. 570, and xii. (1885), p. 31.

¶ *Depth of the Sea* (2nd ed., 1874), p. 500.

arrangement. Their inadequacy for the work led to the construction of entirely novel forms for the "Challenger."[*] The stop-cock water-bottle is a cylinder with a large stop-cock at each end, the levers of which are connected by an outside bar carrying a plate so arranged that it becomes horizontal when the line is hauled in, closes the stop-cocks by its resistance, then falls and hangs freely.

In the water-bottles already described water traverses the apparatus while it is being lowered, and as the openings are smaller than the diameter of the cylinders, the water enclosed at any time is a mixture derived from the few fathoms just passed through. This is no objection in deep-sea work, provided an adequate locking arrangement is employed. When a simple valved cylinder is used the results are untrustworthy, because any check on the line opens the valves and allows the water to get mixed.

The oldest method of getting water from a definite depth was by sinking a corked bottle full of air. It was sent down 100 fathoms or so, and when hauled up it was found still corked, but full of water. This is accounted for by two theories; the water either filters through the cork, or, at a certain depth, the pressure suffices to drive the cork in, the bottle instantly fills, and as it is drawn up, the contained water being relieved from pressure, expands and fixes the cork firmly in its place. This method of collecting water has been employed by many chemists, by Forchhammer[†] and Von Bibra[‡] in particular; but it is very unsatisfactory.

Sir Humphry Davy invented, for Sir John Ross's Arctic voyage of 1818, a water-bottle[§] which was found to be uncertain in its working.[||] It was a strong pear-shaped copper vessel, with a stop-cock at the upper end, which was opened, and, after the bottle filled, closed again by levers actuated by a piston working in an air-tight side-tube, and capable of adjustment enabling it to act at any depth from 5 to 80 fathoms. The arrangement was entirely automatic, and as the pressure at the required depth opened and closed the bottle it was even unnecessary to measure the line as it was payed out.

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[*] Described and figured, *Chall. Rep. Narrative*, i. (1884), pt. 2, p. 3.

[†] *Phil. Trans.*, clv. (1865), p. 203.

[‡] Liebig's *Annalen*, lxxvii. (1851), p. 90.

[§] Described and figured, *Jour. Scien. and the Arts*, v. (1818), p. 231.

[||] Voyage of Discovery in H.M. ships "Isabella" and "Alexander," 1819, Appendix.

In very shallow water a bottle provided with a glass stopper may be sunk, and the stopper pulled out by a second line when at the proper depth. Stevenson's hydrophore (1813)[*] is an improvement on this. It is a metallic bottle closed by a plug, which may be removed and replaced at any depth, in order to fill the vessel and to secure the sample unchanged. Stevenson's deep-sea hydrophore[†] resembles one of Marcet's forms. It is a spindle-shaped vessel, fitted with two conical valves on a very heavy axis, which projects beneath the apparatus and keeps it shut during descent by its weight. When it strikes the bottom the valves are forced up, the air escapes, and water enters. On raising, the valves fall back into their places, and do not tend to open.

Jacobsen[‡] employed, during the cruise of the "Pommerania," in 1872, a glass cylinder adequately protected and provided with weighted valves which hang open during descent; at the required depth it is inverted by a second line, and the weights pressing on the valves serve to secure them. The difficulty of employing a double line is so great, when the sea is rough or when currents are rapid, that apparatus requiring such fitting is very inconvenient.

The Commission at Kiel, for the physical, chemical, and biological examination of the German seas, has devised many methods of collecting water.[§] When the analysis of the dissolved gases in the sample was contemplated, an india-rubber bag containing a little mercury and squeezed free from air, was sunk and opened at the proper position.

On the French "Travailleur" expedition of 1881, a water-bottle combining the peculiarities of the stop-cock and valved forms was employed.[‖] On the "Talisman" expeditions a strong glass globe was exhausted by an air-pump, and its capillary opening sealed; it was fixed to the line below the Negretti-and-Zambra thermometer, which when reversed broke the tip of the capillary tube and water entered the globe. When the globe was received on deck the water sample was permanently secured by sealing off the broken capillary. No other process of collecting and preserving samples of water is so perfect theoretically as this, provided the depth is not too great for the resistance of the vacuous globe to pressure.

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[*] Described and figured, David Stevenson's *Canal and River Engineering* (1872), p. 125.

[†] Described and figured *ibid.*, p. 128.

[‡] Liebig's *Annalen*, clxvii. (1873), p. 1.

[§] See their annual *Berichte, passim.*

[‖] *La Nature*, 1882, i. 53.

Rung has an ingenious water-bottle in the shape of a large syringe [*] with a weighted handle (inside which there is a reversing thermometer); it is sunk with the nozzle downwards, the handle being looped up to a spring hook, which disengages it when a weight is slipped down the line from the ship. The syringe is then inverted, the heavy handle draws out the piston, and water enters through the small hole of the nozzle.

The water-bottles most usually employed during the last fifteen years are made on a principle altogether different from any of those enumerated above. Ekman's apparatus was used for work in shallow water by the Norwegian North Atlantic Expedition. It consisted [†] of a framework like the stand for an hour-glass, the bottom being a brass plate with an india-rubber ring let into it. A cylinder, open below and provided with a flanged opening above, was suspended by a catch to the upper part of the frame, and the whole was lowered over the side. On striking the water the catch fell back, but the rush of water kept the cylinder at the upper part of the frame work; as soon as the line was checked the cylinder fell, its base rested on the india-rubber ring, against which it was pressed by a spring catch.

H. A. Meyer's slip water-bottle [‡] was used on the German North Sea Expedition of 1872, and bottles of identical character, at least so far as regards their main features, were constructed for the "Challenger." [§] This form is practically Marcet's inverted. Instead of consisting of a cylinder with a rod bearing two conical valves hung by a spring above it, it consists of a rod with two conical valves and a cylinder hung above them. In Meyer's arrangement the slip cylinder was hung by a cord to a tumbler, developed from that of Brooke's sounding-rod, and the inner edges at each end of it were ground to fit on the conical edges of the base plate and the upper plate. Strong metal guides fixed to the shank supporting the base plate ensured that the cylinder should slip fairly on its fittings. On striking the bottom the tumbler threw off the slip, and the bottle closed. The "Challenger" instrument worked in the same way. To slip the cylinder at intermediate depths the "Challenger" investigators used a plate resembling that employed for closing the stop-cock bottle, and Meyer adopted a device better adapted for working in shallow water. He suspended the slip-cylinder by a cord and two hooks to two horizontal pins fixed to the upper part of the apparatus. A split wedge attached by rods to a ring surrounding the sounding-line rode on each pin; when a weight was let down the line from the ship it depressed the ring, which lowered the wedges and pushed the hooks off the pins, so closing the apparatus.

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[*] Described and figured, *Dentekniske Forenings Tidskrift*, 1883.

[†] Described and figured, *Norske Nordhavs Expedition*, iv. (1880), pt. ii. p. 17.

[‡] Described and figured, *Exped. zur phys.-chem. und biol. Untersuch. der Nordsee im Sommer* 1872 (1875), p. 4.

[§] For full particulars and figures of the "Challenger" water-bottles, with Mr Buchanan's more recent improvements, and of Mr Buchanan's combined sounding-rod and water-bottle, see *Challenger Rep. Narrative*, vol. i. pt. i. p. 111.

The superiority of slip water-bottles lies in their being adapted to take water from a perfectly definite position. In fact, a slip water-bottle, when it acts properly, cuts out of the layer in which it closes a cylinder of water, the parts of which retain their relative positions until the water-bottle is emptied.

In Buchanan's small slip water-bottle* the cylinder is allowed to run down the line from the ship. Its defects are its aptness to indent the edge by striking on the brass hemisphere, and its being too light to press sufficiently on the rubber to ensure that its contents are unmixed. It cannot, of course, be used on the same line with thermometers. This instrument was at first used in the work carried on by the Scottish Marine Station on the Firth of Forth, where water samples have to be obtained at all depths from 2 fathoms to 40, sometimes in a strong current, sometimes in a rather rough sea, and sometimes in a narrow channel, where there is considerable traffic, and where the instruments cannot be left down for many minutes. It was not very well suited for this work; for which it was found desirable to have a water-bottle fulfilling the following conditions:—

1. To be light enough to work by hand when necessary.
2. To contain about 2 litres of water.
3. To be used on the same line with thermometers.

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* Described and figured, *Proc. Roy. Soc. Edin.*, xiii. (1885), p. 32.

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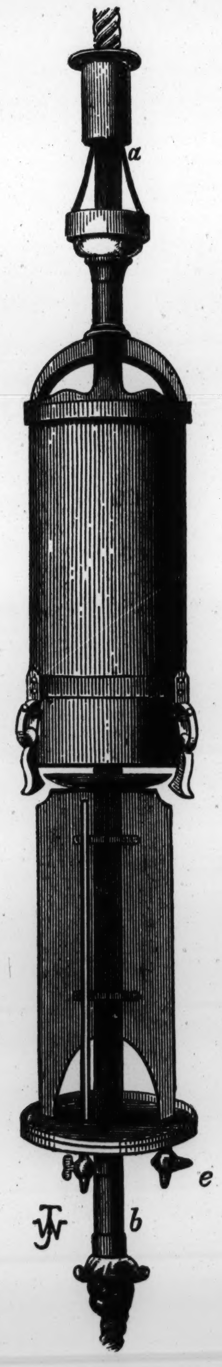

FIG. 1.

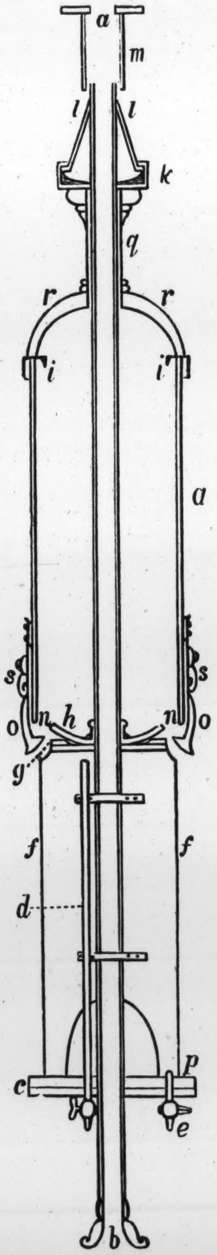

FIG. 2.

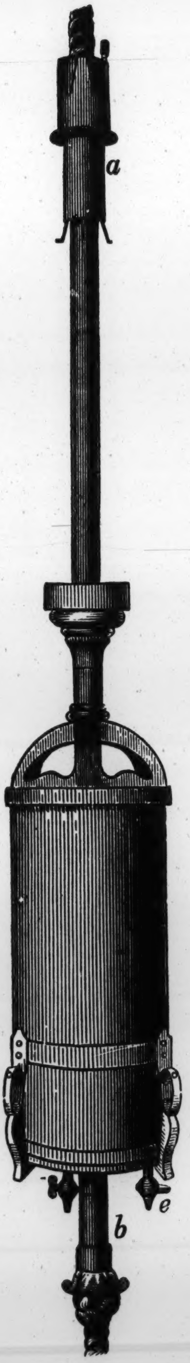

FIG. 3.

4. To be closed at pleasure at any depth, or to be hauled up open if required.

5. To hold its charge perfectly secure.

6. To let the observer know when it closes.

When it was decided to have such an instrument made, I was not acquainted with the description of Meyer's water-bottle, or any other approximately answering the requirements; as I had not then time to search out the scattered papers on the subject, and as no list of references could be got, the water-bottle shown to the meeting was devised, and by many experiments, with the advice of Mr Buchanan, and the assistance of Mr Frazer, who made the instrument, it was brought to a satisfactory condition.

The water-bottle is represented in figs. 1 to 3, Plate XX. Fig. 1 gives its general appearance when open and ready for use; fig. 2 is a vertical section; fig. 3 shows it closed; fig. 4 is the plan of the base-plate. The material of the whole is brass, with the exception of an india-rubber ring and washer. The axis *ab* (see Plate XX.) is a strong tube which supports all the fittings, and through which the sounding-line passes. The funnel-shaped end of the tube *b* rests either on a knot, or on a short bar spliced into the rope. The base-plate C is provided with a ring *p*, of peculiarly soft rubber, specially prepared for the purpose. It is perforated by the stop-cock *e*, which opens by a key to run off the water when the bottle is full, and by the tube *d*, also terminating in a stop-cock, and intended to let in air when the bottle is being emptied. Three light radial wings *ff* serve as guides to the slip cylinder. The top plate *g* has a saucer-shaped india-rubber washer *h* fixed on it by a screw. The slip cylinder has its lower edge *nn* lightly grooved, so as to grip the rubber ring, and a flange with a blunt knife-edge *ii* projects from the upper part. Three strong curved strips *rr* terminate above in a tube *q*, sliding very easily on the central rod. The upper part of *q* is widened, and is provided with an internal groove *k*, in which the bent extremities of three springs *ll* catch, and so support the slip. These springs are bent outwards, and when the bottle is to be

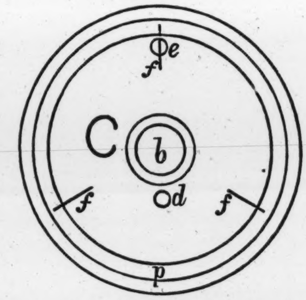

Fig. 4.

closed, a weight slipped down the line strikes the wide tube *m*, and drives it down over the springs *ll*, drawing them in and releasing the slip. The knife-edge *ii* strikes the rubber washer *h* at the same moment as the lower edge *n* strikes and indents the rubber ring *p*. At the same instant the three springs *oo* are forced over the base-plate, and clasp it tightly, clamping the two together before the elasticity of the india-rubber has time to assert itself. An extremely tightly-fitting joint is the result. The spring *o* is supplemented by an additional spring *s*, the action of which enables more flexible metal to be used for the catches. It requires a little practice to enable one to set the water-bottle again, but this is soon acquired.

This water-bottle is believed to possess certain advantages over other forms. It can be used in conjunction with the reversing thermometers of Messrs Negretti and Zambra, when these are fitted with the Scottish frame,* and by a very easy adjustment any number of thermometers and water-bottles can be used simultaneously on one line, thus enabling samples of water and temperature observations to be obtained from several positions at once. The impact of the messenger on the top of the water-bottle, and the locking of the springs below, can be felt distinctly through nearly 100 fathoms of line, and so the moment when it shuts is known. The locking arrangement is very satisfactory, and so simple that any accident, such as the distortion of a spring, can be put right at once. The same holds good of the detaching gear.

The water-bottle shown has been in constant use for a year, both on board the "Medusa," where it is worked by steam-power, and on fishing-boats, where it was managed by hand.

5. On a Repetition of Berthelot's Experiment on the Tenacity of Water and its Adhesion to Glass. By H. Creelman. Communicated by Professor Tait.

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* *Proc. Roy. Soc. Edin.*, xii. (1884), p. 627; xiii. p. 33.

Monday, 1st February 1886.

JOHN MURRAY, Esq., Ph.D., Vice-President, in the Chair.

The following Communications were read:—

1. The Theory of Determinants in the Historical Order of its Development. By Thomas Muir, M.A., LL.D.

Part I.—*Determinants in General* (1693–1779).

In October 1881 I published in the *Quarterly Journal of Mathematics* (xviii. pp. 110–149) a "List of Writings on Determinants," which contained the titles of all the books, pamphlets, memoirs, magazine articles, &c., which were then known to me to exist on the subject of the Theory of Determinants. The list consisted of 489 entries arranged in chronological order, the first date being 1693, and the last 1880. During the three years which have elapsed since it was published, I have been steadily making manuscript additions to it, not merely in the way of continuation for the purpose of keeping it up to date, but also by the intercalation of omitted titles unearthed in the course of my own researches, or brought to my notice by obliging correspondents.

The continuation of the list from 1880 forwards is comparatively an easy matter: it is not by any means easy to render equally complete that portion of the list which pertains to the eighteenth century. In the early history of a scientific subject, before the nomenclature has become fixed, the mere *titles* of writings are insufficient guides: the searcher's work is, consequently, minute and laborious, and he never can be quite sure that his labours are at an end. As far, however, as Determinants are concerned, I am inclined now to think that the writings which are unknown cannot be of much importance, and that the time has come for using the collected material in the production of a detailed history of the subject.

The plan proposed to be followed is not to give one connected history of determinants as a whole, but to give separately the history of each of the sections into which the subject has been divided, viz., to deal with determinants in general, and thereafter in order with the various special forms. This will not only tend to smoothness in the narrative by doing away with the necessity of frequent harkings back, but it will also be of material importance to investigators who may wish to find out what has already been done in advancing any particular department of the subject. To this end, also, each new result as it appears will be numbered in Roman figures; and if the same result be obtained in a different way, or be generalised, by a subsequent worker, it will be marked among the contributions of the latter with the same Roman figures, followed by an Arabic numeral. Thus the theorem regarding the effect of the transposition of two rows of a determinant will be found under Vandermonde, marked with the number xi., and the information intended thus to be conveyed is that in the order of discovery the said theorem was the *eleventh* noteworthy result obtained: while the mark XI. 2, which occurs under Laplace, is meant to show that the theorem was not then heard of for the first time, but that Laplace contributed something additional to our knowledge of it. In this way any reader who will take the trouble to look up the sequence XI., XI. 2, XI. 3, &c., may be certain, it is hoped, of obtaining the full history of the theorem in question.

The early foreshadowings of a new domain of science, and tentative gropings at a theory of it, are so difficult for the historian to represent without either conveying too much or too little, that the only satisfactory way of dealing with a subject in its earliest stages seems to be to reproduce the exact words of the authors where essential parts of the theory are concerned. This I have resolved to do, although to some it may have the effect of rendering the account at the commencement somewhat dry and forbidding.

No author, so far as I am aware, has preceded me in the task I have chosen. Sketches of the history have appeared in a number of text-books of the subject, notably in Günther's *Lehrbuch der Determinanten-Theorie für Studirende* (2te Aufl. xii. 209 pp., Erlangen, 1877), which contains a considerable quantity of detail. The *early* history has been very carefully dealt with by F. J. Studnička, in a memoir published in the *Abhandlungen der königl. böhm. Gesellschaft der Wissenschaften*, 6 Folge, viii. 40 pp. (24th March 1876), and entitled "A. L. Cauchy als formaler Begründer der Determinanten-Theorie. Eine literarisch-historische Studie." There is also an academic thesis (*Teorin för Determinant-Kalkylen*, 121 pp., Helsingfors, 1st March 1876), by E. J. Mellberg, which treats somewhat at length of the early authorities. The existence of these two latter writings has not, however, induced me to curtail to any extent the corresponding part of my work.

LEIBNITZ (1693).

[Leibnizen's mathematische Schriften, herausg. v. C. I. Gerhardt. 1 Abth. ii. pp. 229, 238–240, 245, Berlin, 1850.]

In the fourth letter of the published correspondence between Leibnitz and De L'Hospital, the former incidentally mentions that in his algebraical investigations he occasionally uses numbers instead of letters, treating the numbers however as if they were letters. De L'Hospital, in his reply, refers to this, stating that he has some difficulty in believing that numbers can be as convenient or give as general results as letters. Thereupon Leibnitz, in his next letter (28th April 1693), proceeds with an explanation :—

> "Puisque vous dites que vous avés de la peine à croire qu'il soit aussi general et aussi commode de se servir des nombres que des lettres, il faut que je ne me sois pas bien expliqué. On ne sçauroit douter de la generalité en considerant qu'il est permis de se servir de 2, 3, etc., comme d' *a* ou de *b*, pour veu qu'on considere que ce ne sont pas de nombres veritables. Ainsi 2.3 ne signifie point 6 mais autant qu' *ab*. Pour ce qui est de la commodité, il y en a des très grandes, ce qui fait que je m'en sers souvent, sur tout dans les calculs longs et difficiles ou il est aisé de se tromper. Car outre la commodité de l'épreuve par des nombres, et même par l'abjection du novenaire, j' y trouve un tres grand avantage même pour l'avancement de l'Analyse. Comme c'est une ouverture assez extraordinaire, je n'en ay pas encor parlé à d'autres, mais voicy ce que c'est. Lorsqu'on a besoin de beaucoup de lettres, n'est il pas vray que ces lettres n'expriment point les rapports qu'il y a entre les grandeurs qu'elles signifient, au lieu qu'en me servant des nombres je puis exprimer ce rapport. Par exemple soyent proposées trois equations simples pour deux inconnues à dessein d'oster ces deux inconnues, et cela par un canon general. Je suppose

$$
\begin{array}{lll}
 & 10+11x+12y=0 & (1)\\
\text{et} & 20+21x+22y=0 & (2)\\
\text{et} & 30+31x+32y=0 & (3)
\end{array}
$$

ou le nombre feint estant de deux characteres, le premier me marque de quelle equation il est, le second me marque à quelle lettre il appartient. Ainsi en calculant on trouve par tout des harmonies qui non seulement nous servent de garans, mais encor nous font entrevoir d'abord des regles ou theoremes. Par exemple ostant premierement y par la premiere et la seconde equation, nous aurons:

$$
\begin{array}{l}
+10.22+11.22x\\
-12.20-12.21..
\end{array}
=0 \qquad (4)[*]
$$

et par la premiere et troisieme nous aurons:

$$
\begin{array}{l}
+10.32+11.32x\\
-12.30-12.31..
\end{array}
=0 \qquad (5)
$$

ou il est aise de connoistre que ces deux equations ne different qu'en ce que le charactere antecedent 2 est changé au charactere antecedent 3. Du reste, dans un même terme d'une même equation les characteres antecedens sont les mêmes, et les characteres posterieurs font une même somme. Il reste maintenant d'oster la lettre x par la quatrieme et cinquieme equation, et pour cet effect nous aurons [†]

$$
\begin{array}{lcl}
1_0.2_1.3_2 & & 1_0.2_2.3_1\\
1_1.2_2.3_0 & = & 1_1.2_0.3_2\\
1_2.2_0.3_1 & & 1_2.2_1.3_0
\end{array}
$$

qui est la derniere equation delivrée des deux inconnues qu'on vouloit oster, et qui porte sa preuve avec soy par les harmonies qui se remarquent par tout, et qu'on auroit bien de la peine à decouvrir en employant des lettres a, b, c, sur tout

---

[*] This is written shortly for

$$
\left.\begin{array}{l}
+10.22+11.22x=0\\
-12.20-12.21x=0
\end{array}\right\}
$$

[†] The author here slightly changes his notation. What is meant to be indicated is

$$10.21.32+11.22.30+12.20.31=10.22.31+11.20.32+12.21.30.$$

lors que le nombre des lettres et des equations est grand. Une partie du secret de l'analyse consiste dans la caracteristique, c'est à dire dans l'art de bien employer les notes dont on se sert, et vous voyés, Monsieur, par ce petit echantillon, que Viete et des Cartes n'en ont pas encor connu tous les mysteres. En poursuivant tant soit peu ce calcul on viendra à un *theoreme general* pour quelque nombre de lettres et d'equations simples qu'on puisse prendre. Le voicy comme je l'ay trouvé autres fois:

"*Datis aequationibus quotcunque sufficientibus ad tollendas quantitates, quae simplicem gradum non egrediuntur, pro aequatione prodeunte, primo sumendae sunt omnes combinationes possibiles, quas ingreditur una tantum coefficiens uniuscujusque aequationis: secundo, eae combinationes opposita habent signa, si in eodem aequationis prodeuntis latere ponantur, quae habent tot coefficientes communes, quot sunt unitates in numero quantitatum tollendarum unitate minuto: caeterae habent eadem signa.*

"J'avoue que dans ce cas des degrés simples on auroit peut estre decouvert le même theoreme en ne se servant que de lettres à l'ordinaire, mais non pas si aisement, et ces adresses sont encor bien plus necessaires pour decouvrir des theoremes qui servent à oster les inconnues montées à des degrés plus hauts. Par exemple, ... "

It will be seen that what this amounts to is *the formation of a rule for writing out the resultant of a set of linear equations.* When the problem is presented of eliminating x and y from the equations

$$a+bx+cy=0\,, \quad d+ex+fy=0\,, \quad g+hx+ky=0,$$

Leibnitz in effect says that first of all he prefers to write 10 for a, 11 for b, and so on; that, having done this, he can all the more readily take the next step, viz., forming every possible product whose factors are one coefficient from each equation,* the result being

$$10.21.32\,,\quad 10.22.31\,,\quad 11.20.32\,,$$
$$11.22.30\,,\quad 12.20.31\,,\quad 12.21.30\,;$$

and that, then, *one* being the number which is less by one than the number of unknowns, he makes those terms different in sign which have only *one* factor in common.

---

* Of course, this is not exactly what Leibnitz meant to say.

The contributions, therefore, which Leibnitz here makes to algebra may be looked upon as three in number:—

(1) A *new notation*, numerical in character and appearance, for individual members of an arranged group of magnitudes; the two numbers which constitute the notation being like the Cartesian co-ordinates of a point in that they denote any one of the said magnitudes by indicating its position in the group, . . . (I.)

(2) A rule for *forming the terms* of the expression which equated to zero is the result of eliminating the unknowns from a set of simple equations, ... (II.)

(3) A rule for *determining the signs* of the terms in the said result. (III.)

The last of these is manifestly the least satisfactory. In the first place, part of it is awkwardly stated. Making those terms different in sign *which have only as many factors alike as is indicated by the number which is less by one than the number of unknown quantities* is exactly the same as making those terms different in sign *which have only two factors different.* Secondly, in form it is very unpractical. The only methodical way of putting it in use is to select a term and make it positive; then seek out a second term, having all its factors except two the same as those of the first term, and make this second term negative; then seek out a third term, having all its factors except two the same as those of the second term, and make this third term positive; and so on.

Although there is evidence that Leibnitz continued, in his analytical work, to use his new notation for the coefficients of an equation (see Letters xi., xii., xiii. of the said correspondence), and that he thought highly of it (see Letter viii. "chez moi c'est une des meilleures ouvertures en Analyse"), it does not appear that by using it in connection with sets of linear equations, or by any other means, he went further on the way towards the subject with which we are concerned. Moreover, it must be remembered that the little he did effect had no influence on succeeding workers. So far as is known, the passage above quoted from his correspondence with De L'Hospital was not published until 1850. Even for some little time after the date of Gerhardt's publication it escaped observation, Lejeune Dirichlet being the first to note its historical importance. It is true that during his own lifetime, Leibnitz's *use of numbers in place of letters* was made known to the world in the *Acta Eruditorum* of Leipzig for the year 1700 (*Responsio ad Dn. Nic. Fatii Duillerii imputationes*, pp. 189–208); but the particular application of the new symbols which brings them into connection with determinants was not there given.

CRAMER (1750).

[Introduction a l'Analyse des Lignes Courbes algébriques, par Gabriel Cramer, pp. 59, 60, 656–659. Génève, 1750.]

The third chapter of Cramer's famous treatise deals with the different *orders* (degrees) of curves, and one of the earliest theorems of the chapter is the well-known one that the equation of a curve of the nth degree is determinable when $\frac{1}{2}n(n+3)$ points of the curve are known. In illustration of this theorem he deals (p. 59) with the case of finding the equation of the curve of the *second* degree which passes through *five* given points. The equation is taken in the form

$$A + By + Cx + Dyy + Exy + xx = 0\,;$$

the five equations for the determination of A, B, C, D, E are written down; and it is pointed out that all that is necessary is the solution of the set of five equations, and the substitution of the values of A, B, C, D, E thus found. "Le calcul véritablement en seroit assez long," he says; but in a footnote there is the remark that it is to algebra we must look for the means of shortening the process, and we are directed to the appendix for a convenient general rule which he had discovered for obtaining the solution of a set of equations of this kind. The following is the essential part of the passage in which the rule occurs:—

"Soient plusieurs inconnues z, y, x, v, &c., et autant d'équations

$$\begin{gathered} A^1 = Z^1z + Y^1y + X^1x + V^1v + \&c. \\ A^2 = Z^2z + Y^2y + X^2x + V^2v + \&c. \\ A^3 = Z^3z + Y^3y + X^3x + V^3v + \&c. \\ A^4 = Z^4z + Y^4y + X^4x + V^4v + \&c. \\ \&c. \end{gathered}$$

où les lettres A^1, A^2, A^3, A^4, &c., ne marquent, pas comme à l'ordinaire, les puissances d' A, mais le prémier membre, supposé connu, de la prémiére, seconde, troisiéme, quatriéme, &c. équation."

[Here the solutions of the cases of 1, 2, and 3 unknowns are given, and he then proceeds.]

" L'examen de ces Formules fournit cette Règle générale. Le nombre des équations et des inconnues étant *n*, on trouvera la valeur de chaque inconnue en formant *n* fractions dont le dénominateur commun à autant de termes qu'il y a de divers arrangements de *n* choses différentes. Chaque terme est composé des lettres ZYXV, &c., toujours écrites dans le même ordre, mais auxquelles on distribue, comme exposants, les *n* prémiers chiffres rangés en toutes les maniéres possibles. Ainsi, lorsqu'on a trois inconnues, le dénominateur a [$1 \times 2 \times 3 =$] 6 termes, composés des trois lettres ZYX, qui recoivent successivement les exposants 123, 132, 213, 231, 312, 321. On donne à ces termes les signes + ou −, selon la Règle suivante. Quand un exposant est suivi dans le même terme, médiatement ou immédiatement, d'un exposant plus petit que lui, j'appellerai cela un *dérangement*. Qu'on compte, pour chaque terme, le nombre des dérangements : s'il est pair ou nul, le terme aura le signe +; s'il est impair, le terme aura le signe −. Par ex. dans le terme $Z^1Y^2V^3$ il n'y a aucun dérangement; ce terme aura donc le signe +. Le terme $Z^3Y^1X^2$ a aussi le signe +, parce qu'il a deux dérangements, 3 avant 1 et 3 avant 2. Mais le terme $Z^3Y^2X^1$, qui a trois dérangements, 3 avant 2, 3 avant 1, et 2 avant 1, aura le signe −.

"Le dénominateur commun étant ainsi formé, on aura la valeur de *z* en donnant à ce dénominateur le numérateur qui se forme en changeant, dans tous ces termes, Z en A. Et la valeur d' *y* est la fraction qui a le même dénominateur et pour numérateur la quantité qui résulte quand on change Y en A, dans tous les termes du dénominateur. Et on trouve d'une maniére semblable la valeur des autres inconnues."

It is evident at once that the new results here given are—

(1) A rule for *forming the terms* of the common denominator of

the fractions which express the values of the unknowns in a set of linear equations, ... (IV.)

(2) A rule for *determining the sign* of any individual term in the said common denominator (and, included in the rule, the notion of a "dérangement"), ... (III. 2)

(3) A rule for *obtaining the numerators* from the expression for common denominator, ... (V.)

The problem which Cramer set himself at this point in his book was exactly that which Leibnitz had solved, viz., the elimination of n quantities from a set of $n+1$ linear equations. The solution which Cramer obtained, and which, be it remarked, was the solution best adapted for his purpose, was quite distinct in character from that of Leibnitz. Leibnitz gave a rule for writing out the final result of the elimination; what Cramer gives is a rule for writing out the values of the n unknowns as determined from n of the $n+1$ equations, after which we have got to substitute these values in the remaining $(n+1)$th equation. The notable point in regard to the two solutions is, that Cramer's rule for writing the *common denominator* of the values of the n unknowns (an expression of the nth degree in the coefficients) is exactly Leibnitz's rule for writing the *final result*, which is an expression of the $(n+1)$th degree. Had either discoverer been aware that the same rule sufficed for obtaining both of these expressions, he could not have failed, one would think, to note the *recurrent* law of formation of them. The result of eliminating w, x, y, z from the equations,

$$a_rw + b_rx + c_ry + d_rz = e_r \quad (r = 1,\ 2,\ 3,\ 4,\ 5)$$

is, according to Leibnitz, if we embody his rule in a later symbolism,

$$| a_1b_2c_3d_4e_5 | = 0\,;$$

whereas, according to Cramer, it is—

$$a_1\frac{|e_2b_3c_4d_5|}{|a_2b_3c_4d_5|} + b_1\frac{|a_2e_3c_4d_5|}{|a_2b_3c_4d_5|} + c_1\frac{|a_2b_3e_4d_5|}{|a_2b_3c_4d_5|} + d_1\frac{|a_2b_3c_4e_5|}{|a_2b_3c_4d_5|} = e_1,$$

and from the collocation of these the one natural step is to the identity

$$-|a_1b_2c_3d_4e_5| = a_1|e_2b_3c_4d_5| + b_1|a_2e_3c_4d_5| + \cdot\ \cdot\ \cdot\ \cdot - e_1|a_2b_3c_4d_5|\,.$$

The fate of Cramer's rule was very different from that of Leibnitz.

It was soon taken up, and after a time found its way into the schools, where it continued for many years to be taught as the nutshell form of the theory of the solution of simultaneous linear equations. Indeed Gergonne is reported* to have said, "Cette methode était tellement en faveur, que les examens aux écoles des services publics ne roulaient, pour ainsi dire, que sur elle ; on était admis ou rejeté suivant qu'on la possedait bien ou mal."

Finally, the exact difference between Cramer's notation for the coefficients of the unknowns and the notation of Leibnitz should be noted, and in connection therewith the fact that when dealing with the subject of elimination between two equations of the *m*th and *n*th degrees in x Cramer uses a notation closely resembling that which Leibnitz employed, viz. $[1^2]$ $[1^3]$, &c.

BÉZOUT (1764).

[Recherches sur le degré des équations résultantes de l'évanouissement des inconnues, et sur les moyens qu'il convient d'employer pour trouver ces équations.—*Hist. de l'Acad. Roy. des Sciences*, Ann. 1764 (pp. 288–338), pp. 291–295.]

The object of Bézout's memoir is sufficiently apparent from the title; we may therefore at once give those portions of it which directly concern our subject. On p. 291 is the commencement of the following passage:—

> "M. Cramer a donné une règle générale pour les exprimer toutes débarrassées de ce facteur: j'aurois pu m'en tenir à cette règle; mais l'usage m'a fait connoître que quoiqu'elle soit assez simple, quant aux lettres, elle ne l'est pas de même à l'égard des signes lorsqu'on a au-delà d'un certain nombre d'inconnues à calculer; . . . .

Lemme I.

> "Si l'on a un nombre n d'équations du premier degré qui renferment chacune un pareil nombre d'inconnues, sans aucun terme absolument connu, on trouvera par la règle suivante la relation que doivent avoir les coëfficiens de ces inconnues pour que toutes ces équations aient lieu.

---

* By Studnička.

"Soient a, b, c, d, &c., les coëfficiens de ces inconnues dans la première équation.

a', b', c', d', &c., les coëfficiens des mêmes inconnues dans la seconde équation.

a'', b'', c'', d'', &c., ceux de la troisième & ainsi de suite.

"Formez les deux permutations ab & ba & écrivez $ab-ba$; avec ces deux permutations & la lettre c formez toutes les permutations possibles, en observant de changez de signe toutes les fois que c changera de place dans ab & la même chose à l'égard de ba; vous aurez

$$abc-acb+cab-bac+bca-cba.$$

Avec ces six permutations & la lettre d, formez toutes les permutations possibles, en observant de changer de signe à chaque fois que d changera de place dans un même terme; vous aurez

$$\begin{aligned}&abcd-abdc+adbc-dabc-acbd+acdb-adcb+dacb\\&+cabd-cadb+cdab-dcab-bacd+badc-bdac+dbac\\&+bcad-bcda+bdca-dbca-cbad+cbda-cdba+dcba\end{aligned}$$

& ainsi de suite jusqu'à ce que vous ayez épuisé tous les coëfficiens de la première equation.

"Alors conservez les lettres qui occupent la première place; donnez à celles qui occupent la seconde, la même marque qu'elles ont dans la seconde équation; à celles qui occupent la troisième, la même marque qu'elles ont dans la troisième équation, & ainsi de suite; égalez enfin le tout à zéro et vous aurez l'équation de condition cherchée.

"Ainsi si vous avez deux équations et deux inconnues comme

$$\begin{aligned}ax+by&=0\\a'x+b'y&=0\end{aligned}$$

l'équation de condition sera $ab'-ba'=0$ ou $ab'-a'b=0$ . . . ."

In the same way the next two cases are given; then—

". . . . mais comme ces équations de condition doivent servir de formules pour l'élimination dans les équations de différens degrés, il convient de leur donner une forme qui rende les substitutions le moins pénibles qu'il se pourra ; pour cet effet, je les mets sous cette forme :

$$ab' - a'b = 0$$

$$(ab' - a'b)c'' + (a''b - ab'')c' + (a'b'' - a''b')c = 0$$

$$\begin{aligned}
&[(ab' - a'b)c'' + (a''b - ab'')c' + (a'b'' - a''b')c\,]d''' \\
+&[(a'b - ab')c''' + (ab''' - a'''b)c' + (a'''b' - a'b''')c\,]d'' \\
+&[(a'''b - ab''')c'' + (ab'' - a''b)c''' + (a''b''' - a'''b'')c]d' \\
+&[(a'b''' - a'''b')c'' + (a'''b'' - a''b''')c' + (a''b' - a'b'')c''']d = 0.
\end{aligned}$$

Cette nouvelle forme a deux avantages : le premier, de rendre les substitutions à venir, plus commodes ; le deuxième, c'est d'offrir une règle encore plus simple pour la formation de ces formules.

" En effet, il est facile de remarquer 1°, que le premier terme de l'une quelconque de ces équations, est formé du premier membre de l'équation précédente, multiplié par la première des lettres qu'elle ne renferme point, cette lettre étant affectée de la marque qui suit immédiatement la plus haute de celles qui entrent dans ce même membre.

" 2°. Le deuxième terme se forme du premier, en changeant dans celui-ci la plus haute marque en celle qui est immédiatement au-dessous & réciproquement, and de plus en changeant les signes.

" 3°. Le troisième, se forme du premier, en changeant dans celui-ci la plus haute marque en celle de deux numéros au-dessous & réciproquement, & de plus en changeant les signes.

" 4°. Le quatrième, se forme du premier, en changeant dans celui-ci la plus haute marque en celle de trois numéros au-dessous & réciproquement, & changeant les signes, & toujours de même pour les suivans.

" Par exemple, ...

" D'après ces observations, il sera facile de voir que l'équation de condition pour cinq inconnues et cinq équations, sera

. . . ."

The latter part of this we are drawn to at once, as it enunciates quite clearly the Recurrent Law of Formation to which attention has above been directed as a natural deduction from the work of Leibnitz and Cramer.

The notable point in regard to the earlier portion is, that Bézout throws his rule of term-formation and his rule of signs into one. In the case of finding the resultant of

$$a_r x + b_r y + c_r z = 0 \qquad (r = 1,\ 2,\ 3)$$

his process consists of four steps, viz. :—

$$(1)\ a.$$

$$(2)\ a\,b\ \vdots - b\,a$$

$$(3)\ a\,b\,c\ - a\,c\,b\ + c\,a\,b \vdots - b\,a\,c\ + b\,c\,a\ - c\,b\,a\,.$$

$$(4)\ a_1b_2c_3 - a_1c_2b_3 + c_1a_2b_3 - b_1a_2c_3 + b_1c_2a_3 - c_1b_2a_3\,.$$

The first term of (2) is got from (1) by affixing b, and the second is got from the first by advancing the b one place and changing the sign. The first term of (3) is got from the first term of (2) by affixing c, the second term is got from the first by advancing c a place and changing the sign, and the third is got from the second by advancing c a place and changing the sign; the last three are got from the second term of (2) in the same way as the first three are got from the first term of (2).

It will thus be seen that while Leibnitz and Cramer direct us to find the permutations in any way whatever, and thereafter to fix the sign of each in accordance with a rule, Bézout requires the permutations to be found by a particular process, and attention given to the question of sign throughout all this process, so that when the terms have been found their signs have likewise been determined.

Bézout's contributions to the subject thus are—

(1) A combined rule of term-formation and rule of signs, ... } (II. 2) + (III. 3)
(2) The recurrent law of formation of the new functions, (VI.)

VANDERMONDE (1771).

[Mémoire sur l'élimination. *Hist. de l'Acad. Roy. des Sciences.* Ann. 1772, 2^e^ partie (pp. 516–532).]

This important memoir of Vandermonde and that of Laplace, which is dealt with immediately afterwards, both appear in the

History of the French Academy of Sciences for 1772, Laplace's memoir occupying pp. 267–376, and Vandermonde's pp. 516–532. There is, however, a footnote to the latter, which states that it was read for the first time to the Academy on 12th January 1771.

The part of it which concerns us is the first article, which treats of elimination in the case of equations of the first degree. Vandermonde here writes :—

> "Je suppose que l'on représente par $\overset{1}{1},\ \overset{2}{1},\ \overset{3}{1},$ &c., $\overset{1}{2},\ \overset{2}{2},\ \overset{3}{2},$ &c., $\overset{1}{3},\ \overset{2}{3},\ \overset{3}{3},$ &c., &c., autant de différentes quantités générales, dont l'une quelconque soit $\overset{\alpha}{a,}$ une autre quelconque soit $\overset{\beta}{b,}$ &c., & que le produit des deux soit désigné à l'ordinaire par $\overset{\alpha}{a}.\overset{\beta}{b}.$
>
> "Des deux nombres ordinaux α & a, le premier, par exemple, désignera de quelle équation est pris le coëfficient $\overset{\alpha}{\underset{a,}{\text{-}}}$ & le second désignera le rang que tient ce coëfficient dans l'équation, comme on le verra ci-après.
>
> "Je suppose encore le système suivant d'abréviations, & que l'on fasse

$$\frac{\alpha\,|\,\beta}{a\,|\,b} = \overset{\alpha}{a}.\overset{\beta}{b} - \overset{\alpha}{b}.\overset{\beta}{a},$$

$$\frac{\alpha\,|\,\beta\,|\,\gamma}{a\,|\,b\,|\,c} = \overset{\alpha}{a}.\frac{\beta\,|\,\gamma}{b\,|\,c} + \overset{\alpha}{b}.\frac{\beta\,|\,\gamma}{c\,|\,a} + \overset{\alpha}{c}.\frac{\beta\,|\,\gamma}{a\,|\,b},$$

$$\frac{\alpha\,|\,\beta\,|\,\gamma\,|\,\delta}{a\,|\,b\,|\,c\,|\,d} = \overset{\alpha}{a}.\frac{\beta\,|\,\gamma\,|\,\delta}{b\,|\,c\,|\,d} - \overset{\alpha}{b}.\frac{\beta\,|\,\gamma\,|\,\delta}{c\,|\,d\,|\,a} + \overset{\alpha}{c}.\frac{\beta\,|\,\gamma\,|\,\delta}{d\,|\,a\,|\,b} - \overset{\alpha}{d}\cdot\frac{\beta\,|\,\gamma\,|\,\delta}{a\,|\,b\,|\,c}$$

$$\frac{\alpha\,|\,\beta\,|\,\gamma\,|\,\delta\,|\,\epsilon\,|}{a\,|\,b\,|\,c\,|\,d\,|\,e\,|} = \overset{\alpha}{a}.\frac{\beta\,|\,\gamma\,|\,\delta\,|\,\epsilon}{b\,|\,c\,|\,d\,|\,e} + \ldots\ldots$$

. . . .

> "Le symbole $\frac{\;|\;\;|\;}{\;|\;\;|\;}$ sert ici de caractéristique. Les seules choses à observer sont l'ordre des signes, et la loi des permutations entre les lettres *a*, *b*, *c*, *d*, &c., qui me paroissent suffisamment indiquées ci-dessus.
>
> "Au lieu de transposer les lettres *a*, *b*, *c*, *d*, &c., on pouvoit

les laisser dans l'ordre alphabétique, & transposer au contraire les lettres α, β, γ, δ, &c., les résultats auroient été parfaitement les mêmes; ce qui a lieu aussi par rapport aux conclusions suivantes.

"Premièrement, il est clair que $\frac{\alpha \mid \beta}{a \mid b}$ représente deux termes différens, l'un positif, & l'autre négatif, résultans d'autant de permutations possibles de a & b; que $\frac{\alpha \mid \beta \mid \gamma}{a \mid b \mid c}$ en représente six, trois positifs & trois négatifs, résultans d'autant de permutations possibles de a, b, & c; que $\frac{\alpha \mid \beta \mid \gamma \mid \delta}{a \mid b \mid c \mid d}$ . . . .

"Mais de plus, la formation de ces quantités est telle que l'unique changement que puisse résulter d'une permutation, quelle qu'elle soit, faite entre les lettres du même alphabet, dans l'une de ces abréviations, sera un changement dans le signe de la première valeur.

"La démonstration de cette vérité & la recherche du signe résultant d'une permutation déterminée, dépendent généralement de deux propositions qui peuvent être énoncées ainsi qu'il suit, en se servant de nombres pour indiquer le rang des lettres.

"La première est que

$$\frac{1 \mid 2 \mid 3 \mid \ldots \mid m \mid m+1 \mid \ldots n}{1 \mid 2 \mid 3 \mid \ldots\ m \mid m+1 \mid \ldots n}$$

$$= \pm \frac{1 \mid 2 \mid 3 \mid \ldots \mid n-m+1 \mid n-m+2 \mid n-m+3 \mid \ldots \mid n}{m \mid m+1 \mid m+2 \mid \ldots \mid n \mid 1 \mid 2 \mid \ldots \mid m-1}$$

le signe − n'ayant lieu que dans le cas où n & m sont l'un & l'autre des nombres pairs.

"La seconde est que

$$\frac{1 \mid 2 \mid 3 \mid \ldots \mid m \mid m+1 \mid \ldots \mid n}{1 \mid 2 \mid 3 \mid \ldots \mid m \mid m+1 \mid \ldots \mid n}$$

$$= - \frac{1 \mid 2 \mid 3 \mid \ldots \mid m-1 \mid m \mid m+1 \mid m+2 \mid \ldots \mid n}{1 \mid 2 \mid 3 \mid \ldots \mid m-1 \mid m+1 \mid m \mid m+2 \mid \ldots \mid n}.$$

"Il sera facile de voir que, la première équation supposée, celle-ci n'a besoin d'être prouvée que pour un seul cas, comme, par exemple, celui de $m=n-1$, c'est-à-dire, celui où les deux lettres transposées sont les deux dernières.

“ Au lieu de démontrer généralement ces deux équations, ce qui exigeroit un calcul embarrassant plutôt que difficile, je me contenterai de développer les exemples les plus simples : cela suffira pour saisir l'esprit de la démonstration.

. . . .”

($2\frac{1}{2}$ pages are occupied with verifications for the case of

$$\frac{\alpha\,|\,\beta}{a\,|\,b}, \quad \text{of} \quad \frac{\alpha\,|\,\beta\,|\,\gamma}{a\,|\,b\,|\,c}, \quad \text{and of} \quad \frac{\alpha\,|\,\beta\,|\,\gamma\,|\,\delta}{a\,|\,b\,|\,c\,|\,d}.\Big)$$

“ On verra qu'en général la démonstration de notre seconde équation pour le cas $n=a$, dépend de cette même équation pour le cas $n=a-1$, quel que soit a : d'où il suit que puisque $\frac{1\,|\,2}{1\,|\,2} = -\frac{1\,|\,2}{2\,|\,1}$, elle est généralement vraie.

. . . .

“ De ce que nous avons dit jusqu'ici *il suit que*

$$\frac{\alpha\,|\,\beta\,|\,\gamma\,|\,\delta\,|\,\ldots\ldots}{a\,|\,b\,|\,c\,|\,d\,|\,\ldots\ldots} = 0,$$

si deux lettres quelconques du même alphabet sont égales entr'elles ; car quelque part que soient les deux lettres égales, on peut les transposer aux deux dernières places de leur rang, ce qui ne fera au plus que changer le signe de la valeur ; alors, de leur permutation particulière, il ne peut, d'une part, résulter aucun changement, puisqu'elles sont égales ; d'autre part, selon notre seconde équation ci-dessus, il doit en résulter un changement de signe ; cette contradiction ne peut être levée qu'en supposant la valeur zéro. . . . . . .

“ Tout cela posé ; puisque l'on a identiquement,

$$\frac{1\,|\,1\,|\,2}{1\,|\,2\,|\,3} = \frac{1}{1}\,.\,\frac{1\,|\,2}{2\,|\,3} + \frac{1}{2}\,.\,\frac{1\,|\,2}{3\,|\,1} + \frac{1}{3}\,.\,\frac{1\,|\,2}{1\,|\,2} = 0,$$

$$\frac{2\,|\,1\,|\,2}{1\,|\,2\,|\,3} = \frac{2}{1}\,.\,\frac{1\,|\,2}{2\,|\,3} + \frac{2}{2}\,.\,\frac{1\,|\,2}{3\,|\,1} + \frac{2}{3}\,.\,\frac{1\,|\,2}{1\,|\,2} = 0,$$

si l'on propose de trouver les valeurs de $\xi 1$ et de $\xi 2$ qui satisfont aux deux équations

$$\overset{1}{1}\,.\,\xi 1 + \overset{2}{2}\,.\,\xi 2 + \overset{1}{3} = 0$$

$$\overset{2}{1}\,.\,\xi 1 + \overset{2}{2}\,.\,\xi 2 + \overset{2}{3} = 0,$$

on pourra comparer, & l'on aura

$$\xi 1 = \frac{\frac{1|2}{2|3}}{\frac{1|2}{1|2}}, \quad \xi 2 = \frac{\frac{1|2}{3|1}}{\frac{1|2}{1|2}}.$$

. . . ."

(Three equations with three unknowns similarly dealt with.)

" Il est clair que ces valeurs n'ont point de facteurs inutiles : mais pour les rendre aussi commodes qu'il est possible dans les applications, and particulièrement dans celles où l'on veut faire usage des logarithmes, il sera bon d'y employer le plus qu'il se pourra, la multiplication des facteurs complexes. J'observe donc 1° que si l'on substitue dans le développement de $\frac{\alpha|\beta|\gamma|\delta}{a|b|c|d}$, les valeurs des $\frac{\alpha|\beta|\gamma}{a|b|c}$ en $\frac{\alpha|\beta}{a|b}$, on aura, en réduisant & ordonnant, d'après les observations ci-dessus,

$$\frac{\alpha|\beta|\gamma|\delta}{a|b|c|d} = \left\{ \begin{array}{l} \frac{\alpha|\beta}{a|b}\cdot\frac{\gamma|\delta}{c|d} - \frac{\alpha|\beta}{a|c}\cdot\frac{\gamma|\beta}{b|d} + \frac{\alpha|\beta}{a|d}\cdot\frac{\gamma|\delta}{b|c} \\ + \frac{\alpha|\beta}{b|c}\cdot\frac{\gamma|\delta}{a|d} - \frac{\alpha|\beta}{b|d}\cdot\frac{\gamma|\delta}{a|c} \\ + \frac{\alpha|\beta}{c|d}\cdot\frac{\gamma|\delta}{a|b} \end{array} \right.$$

si de même on substitue dans le développement des $\frac{\alpha|\beta|\gamma|\delta|\epsilon|\zeta}{a|b|c|d|e|f}$, les valeurs des $\frac{\alpha|\beta|\gamma|\delta|\epsilon}{a|b|c|d|e}$ en $\frac{\alpha|\beta|\gamma|\delta}{a|b|c|d}$, on aura, en réduisant & ordonnant, d'après les observations ci-dessus,

$$\frac{\alpha|\beta|\gamma|\delta|\epsilon|\zeta}{a|b|c|d|e|f}=\left\{\begin{array}{l}
\frac{\alpha|\beta}{a|b}\cdot\frac{\gamma|\delta|\epsilon|\zeta}{c|d|e|f}-\frac{\alpha|\beta}{a|c}\cdot\frac{\gamma|\delta|\epsilon|\zeta}{b|d|e|f}+\frac{|}{a|d}\cdot\frac{\;|\;|\;|\;}{b|c|e|f}\\
+\frac{\alpha|\beta}{b|c}\cdot\frac{\gamma|\delta|\epsilon|\zeta}{a|d|e|f}-\frac{|}{b|d}\cdot\frac{\;|\;|\;|\;}{a|c|e|f}+\frac{|}{b|e}\cdot\frac{\;|\;|\;|\;}{a|c|d|f}\\
+\frac{|}{c|d}\cdot\frac{\;|\;|\;|\;}{a|b|e|f}-\frac{|}{c|e}\cdot\frac{\;|\;|\;|\;}{a|b|d|f}+\frac{|}{c|f}\cdot\frac{\;|\;|\;|\;}{a|b|d|e}\\
+\frac{|}{d|e}\cdot\frac{\;|\;|\;|\;}{a|b|c|f}-\frac{|}{d|f}\cdot\frac{\;|\;|\;|\;}{a|b|c|e}\\
+\frac{|}{e|f}\cdot\frac{\;|\;|\;|\;}{a|b|c|d}\\
\qquad\qquad\qquad\qquad-\frac{|}{a|e}\cdot\frac{\;|\;|\;|\;}{b|c|d|f}+\frac{|}{a|f}\cdot\frac{\;|\;|\;|\;}{b|c|d|e}\\
\qquad\qquad\qquad\qquad-\frac{|}{b|f}\cdot\frac{\;|\;|\;|\;}{a|c|d|e}
\end{array}\right.$$

"La loi des permutations & des signes est assez manifeste dans ces exemples, pour qu'on en puisse conclure des développemens pareils pour les cas de huit & dix lettres, &c., du même alphabet; alors, en employant les premiers développemens pour les cas d'un nombre impair de ces lettres, on aura les formules d'élimination du premier degré, sous la forme la plus concise qu'il soit possible.

"Si l'on veut exprimer ces formules, généralement pour un nombre n d'équations

$$\overset{1}{1}.\xi 1+\overset{1}{2}.\xi 2+\overset{1}{3}.\xi 3+\ldots+\overset{1}{m}.\xi m+\ldots+\overset{1}{n}.\xi n+(\overset{1}{n+1})=0$$

$$\overset{2}{1}.\xi 1+\overset{2}{2}.\xi 2+\overset{2}{3}.\xi 3+\ldots+\overset{2}{m}.\xi m+\ldots+\overset{2}{n}.\xi n+(\overset{2}{n+1})=0$$

&c.

la valeur de l'inconnue quelconque ξm, sera renfermée dans l'équation suivante, à une seule inconnue

$$\frac{1\,|\,2\,|\,3\,|\ldots|\,n}{1\,|\,2\,|\,3\,|\ldots|\,n}.\,\xi m$$

$$\pm\frac{1\;|\;2\;|\;3\;|\ldots\ldots|n-m|n-m+1|n-m+2|n-m+3|\ldots|\;n}{m+1|m+2|m+3|\ldots\ldots|\;n\;|\;n+1\;|\;1\;|\;2\;|\;|m-1}=0$$

le signe + ayant lieu seulement dans le cas où m & n sont impairs l'un & l'autre."

Taking this up in order, we observe first that Vandermonde pro-

poses for coefficients a positional notation essentially the same as that of Leibnitz, writing $\frac{1}{2}$ where Leibnitz wrote 12 or 1_2.

Then he defines a certain class of functions by means of their recurrent law of formation—a law and class of functions at once seen to be identical with those of Bézout. A special symbolism is used for the first time to denote the functions; thus, the expression

$$1_0.2_1.3_2+1_1.2_2.3_0+1_2.2_0.3_1-1_0.2_2.3_1-1_1.2_0.3_2-1_2.2_1.3_0,$$

which occurs in Leibnitz's letter, Vandermonde would have denoted by

$$\frac{1\mid 2\mid 3}{1\mid 2\mid 3},$$

and the result of eliminating x, y, z, w from the set of equations

$$1_r x+2_r y+3_r y+4_r w=0 \quad (r=1,2,3,4)$$

by

$$\frac{1\mid 2\mid 3\mid 4}{1\mid 2\mid 3\mid 4}.$$

It is next pointed out that permutation of the under row of indices produces the same result as permutation of the upper row, that the number of terms is the same as the number of permutations of either row of indices, and that half of the terms are positive and half negative.

The part which follows this is a little curious. The proposition is brought forward that if in the symbolism for one of the functions a transposition of indices takes place in either row, the same function is still denoted, the only change thereby possible being a change of sign. The demonstration is affirmed to be dependent on two theorems, neither of which is proved, as the proofs are said to be troublesome to set forth. Now it will be seen that the second of these theorems is to the effect that the transposition of any two consecutive indices causes a change of sign, and that consequently this alone is sufficient for the required demonstration. The first of the auxiliary theorems, in fact, is an immediate deduction from the second, the particular permutation which it concerns being produced by $(n-m+1)(m-1)$ transpositions of pairs of consecutive indices.

Passing over the illustrations of these propositions, we come next to the theorem that if any two indices of either row be equal the function vanishes identically, and we note particularly that the basis of the proof is that the interchange of the two indices in question changes the sign of the function, and yet leaves the function unaltered.

Upon this theorem the solution of a set of simultaneous linear equations is then with much neatness made to depend. In more modern notation Vandermonde's process is as follows:—It is known that

$$a_1\,|\,b_1c_2\,| + b_1\,|\,c_1a_2\,| + c_1\,|\,a_1b_2\,| = |\,a_1b_1c_2\,| = 0,$$

and

$$a_2\,|\,b_1c_2\,| + b_2\,|\,c_1a_2\,| + c_2\,|\,a_1b_2\,| = |\,a_2b_1c_2\,| = 0,$$

$$\left.\begin{array}{ll}\therefore & a_1\dfrac{|\,b_1c_2\,|}{|\,a_1b_2\,|} + b_1\dfrac{|\,c_1a_2\,|}{|\,a_1b_2\,|} + c_1 = 0\\[2ex] \text{and} & a_2\dfrac{|\,b_1c_2\,|}{|\,a_1b_2\,|} + b_2\dfrac{|\,c_1a_2\,|}{|\,a_1b_2\,|} + c_2 = 0\end{array}\right\}$$

hence, if the equations

$$\left.\begin{array}{l}a_1x + b_1y + c_1 = 0\\ a_2x + b_2y + c_2 = 0\end{array}\right\}$$

be given us, we know that

$$x = \frac{|\,b_1c_2\,|}{|\,a_1b_2\,|},\quad y = \frac{|\,c_1a_2\,|}{|\,a_1b_2\,|}$$

is a solution.

This result, moreover, is generalised; the solution of

$$r_1x_1 + r_2x_2 + \ldots. + r_nx_n + r_{n+1} = 0 \qquad (r = 1, 2, \ldots, n)$$

being fully and accurately expressed in symbols, although the numerators of the values of $x_1, x_2, \ldots, x_n$ are not in so simple a form as Cramer's rule for obtaining the numerator from the denominator might have suggested.

Lastly, and almost incidentally, Vandermonde makes known a case of the widely general theorem now-a-days described as the theorem for expressing a determinant as an aggregate of products of complementary minors. His case is that in which the given determinant is of the order $2m$, and one factor of each of the products is of order 2.

Summing up, therefore, we must put the statement of our indebtedness to Vandermonde as follows:—

(1) A simple and appropriate notation for the new functions, *e.g.*, $\frac{1\mid 2\mid 3}{1\mid 2\mid 3}$, ... (VII.)

(2) A new mode of defining the functions, viz., using Bézout's recurring law of formation, ... (VIII.)

(3) The remark that the ordinary algebraical expression of any of the functions is obtainable by permutation of *either* series of indices, ... (IX.)

(4) The remark that the positive and negative terms are equal in number, ... (X.)

(5) The theorem regarding the effect of interchanging two consecutive indices, ... (XI.)

(6) The theorem (with proof) regarding the effect of equality of two indices belonging to the same series, ... (XII.)

(7) A reasoned-out solution of a set of n simultaneous linear equations, by means of the new functions as above defined, . (XIII.)

(8) Expression of any of the new functions of order $2m$ as an aggregate of products of like functions of orders 2 and $2m-2$, ... (XIV.)

In addition to this, we must view Vandermonde's work as a whole, and note that he is the first to give a connected exposition of the theory, defining the functions apart from their connections with other matter, assigning them a notation, and thereafter logically developing their properties. After Vandermonde there could be no absolute necessity for a renovation or reconstruction on a new basis: his successors had only to extend what he had done, and, it might be, to perfect certain points of detail. Of the mathematicians whose work has thus far been passed in review, the only one fit to be viewed as the founder of the theory of determinants is Vandermonde.

LAPLACE (1772).

[Recherches sur le calcul intégral et sur le système du monde. *Hist. de l'Acad. Roy. des Sciences, Ann.* 1772, 2^e *partie* (pp. 267–376) pp. 294–304].

In the course of his work Laplace arrives at a set of linear equations from which n quantities have to be eliminated. This he says can be accomplished by means of rules which mathematicians have given:—

> "Mais comme elles ne me paroissent avoir été jusqu'ici démontrées que par induction, et que d'ailleurs elles sont impracticables, pour peu que le nombre des équations soit considérable; je vais reprendre de nouveau cette matière, et donner quelques procédés plus simples que ceux qui sont déjà connus, pour éliminer entre un nombre quelconque d'équations du premier degré."

Taking n homogeneous linear equations with the coefficients

$$\begin{array}{llll} {}^1a, & {}^1b, & {}^1c, & \ldots \\ {}^2a, & {}^2b, & {}^2c, & \ldots \\ \ldots & \ldots & \ldots & \ldots \end{array}$$

he first gives Cramer's rule for writing out what he, Laplace, calls the *Resultant,* using in the course of the rule the term *variation* instead of Cramer's term "*dérangement.*" Then he gives the "perhaps simpler" rule of Bézout, and shows that of necessity it will lead to the same result as Cramer's.

The theorem in regard to the effect of transposing two letters is next enunciated, and the blank left by Vandermonde is filled, for a proof of the theorem is given. The exact words of the enunciation and proof are—

> "Si au lieu de combiner d'abord la lettre a avec la lettre b, ensuite ces deux-ci avec la lettre c, et ainsi de suite; c'est-à-dire, si au lieu de combiner les lettres a, b, c, d, e, &c., dans l'ordre a, b, c, d, e, &c., on les eût combinées dans l'ordre a, c, b, d, e, &c., ou a, d, b, c, e, &c., ou a, e, b, c, d, &c., ou &c., je dis qu'on auroit toujours eu la même quantité à la différence des signes près.
>
> "Pour démontrer ce Théorème nommons en genéral, *resultante,* la quantité qui résulte de l'une quelconque de ces combinaisons, en sorte que la *première résultante* soit celle qui vient de la combinaison suivant l'ordre a, b, c, d, e, &c., que la *seconde résultante* soit celle qui vient de la combinaison suivant l'ordre a, c, b, d, e, &c., que la *troisième résultante* soit celle qui vient de la combinaison suivant l'ordre a, d, b, c, e, &c., et ainsi de suite; cela posé, il est clair que toutes ces résultantes

renferment le même nombre de termes, et précisément les mêmes, puisqu'elles renferment tous les termes qui peuvent résulter de la combinaison des *n* lettres *a*, *b*, *c*, *d*, *e*, &c., disposées entre elles de toutes les manières possibles; il ne peut donc y avoir de différence entre deux résultantes, que dans les signes de chacun de leurs termes; or, il est visible que la première résultante donne la seconde, si l'on change dans la première *b* en *c*, et réciproquement *c* en *b*; mais ce changement augmente ou diminue d'une unité le nombre des variations de chaque terme; d'ou il suit que dans la seconde résultante, tous les termes dont le nombre des variations est impair, auront le signe +, et les autres le signe −; partant, cette seconde résultante n'est que la première, prise négativement.

"Il est visible pareillement que . . . ." &c.

The proof is thus seen to consist in establishing (1) that the terms of the one "resultant" must, apart from sign, be the same as those of the other; and (2) that the terms of the one resultant are either all affected with the same sign as the like terms of the other, or are all affected with the opposite sign, the comparison of sign being made by comparing the number of variations.

After this, the theorem that when two letters are alike the resultant vanishes is established in a way different from Vandermonde's, but not more satisfactory, viz., by considering what Bézout's rule would lead to in that case.

Application is then made to the problem of elimination, and to the solution of a set of linear simultaneous equations, the mode of treatment being again different from Vandermonde's, but this time with better cause. He says—

"Je suppose maintenant que l'on ait les trois équations

$$\begin{aligned} 0 &= {}^{1}a.\mu + {}^{1}b.\mu' + {}^{1}c.\mu'', \\ 0 &= {}^{2}a.\mu + {}^{2}b.\mu' + {}^{2}c.\mu'', \\ 0 &= {}^{3}a.\mu + {}^{3}b.\mu' + {}^{3}c.\mu'', \end{aligned}$$

je forme d'abord la résultante des trois lettres *a*, *b*, *c*, suivant l'ordre *a*, *b*, *c*, ce qui donne,

$${}^{1}a.{}^{2}b.{}^{3}c - {}^{1}a.{}^{2}c.{}^{3}b + {}^{1}c.{}^{2}a.{}^{3}b - {}^{1}b.{}^{2}a.{}^{3}c + {}^{1}b.{}^{2}c.{}^{3}a - {}^{1}c.{}^{2}b.{}^{3}a.$$

ou

$${}^{1}a.\left[{}^{2}b.{}^{3}c - {}^{2}c.{}^{3}b\right] + {}^{2}a.\left[{}^{1}c.{}^{3}b - {}^{1}b.{}^{3}c\right] + {}^{3}a.\left[{}^{1}b.{}^{2}c - {}^{1}c.{}^{2}b\right];$$

je multiplie ensuite la première des équations précédentes par ${}^2b.{}^3c - {}^2c.{}^3b$, la seconde par ${}^1c.{}^3b - {}^1b.{}^3c$, la troisième par ${}^1b.{}^2c - {}^1c.{}^2b$, et je les ajoute ensemble, ce qui donne,

$$\begin{aligned} 0 = {} & \mu.[{}^1a.({}^2b.{}^3c - {}^2c.{}^3b) + {}^2a.({}^1c.{}^3b - {}^1b.{}^3c) + {}^3a({}^1b.{}^2c - {}^1c.{}^2b)] \\ & + \mu'.[{}^1b.({}^2b.{}^3c - {}^2c.{}^3b) + {}^2b.({}^1c.{}^3b - {}^1b.{}^3c) + {}^3b({}^1b.{}^2c - {}^1c.{}^2b)] \\ & + \mu''.[{}^1c.({}^2b.{}^3c - {}^2c.{}^3b) + {}^2c.({}^1c.{}^3b - {}^1b.{}^3c) + {}^3c({}^1b.{}^2c - {}^1c.{}^2b)]\,; \end{aligned}$$

or, il suit de ce que nous venons de voir, que les coëfficiens de μ' et μ'', sont identiquement nuls, puisqu'ils ne sont que la résultante des trois lettres a, b, c, dans laquelle on écrit b, ou c, par-tout où est a; donc, on aura pour l'équation de condition demandée,

$$0 = {}^1a.({}^2b.{}^3c - {}^2c.{}^3b) + {}^2a.({}^1c.{}^3b - {}^1b.{}^3c) + {}^3a.({}^1b.{}^2c - {}^1c.{}^2b)\,;$$

c'est-à-dire, la résultante de la combinaison des trois lettres a, b, c égalée à zéro. On démontreroit la même chose, quel que soit le nombre des équations."

" Pour montrer l'analogie de cette matière, avec l'élimination des équations du premier degré, je suppose que l'on ait les trois équations,

$$\begin{aligned} {}^1p &= {}^1a.\mu + {}^1b.\mu' + {}^1c.\mu'', \\ {}^2p &= {}^2a.\mu + {}^2b.\mu' + {}^2c.\mu'', \\ {}^3p &= {}^3a.\mu + {}^3b.\mu' + {}^3c.\mu''. \end{aligned}$$

Je multiplie, comme ci-devant, la première par $({}^2b.{}^3c - {}^2c.{}^3b)$, la seconde par $({}^1c.{}^3b - {}^1b.{}^3c)$, et la troisième par $({}^1b.{}^2c - {}^1c.{}^2b)$, je les ajoute ensemble, et j'observe que les coëfficiens de μ' et de μ'', sont identiquement nuls dans l'équation qui en résulte; d'où je conclus,

$$\mu = \frac{{}^1p.({}^2b.{}^3c - {}^2c.{}^3b) + {}^2p.({}^1c.{}^3b - {}^1b.{}^3c) + {}^3p.({}^1b.{}^2c - {}^1c.{}^2b)}{{}^1a({}^2b.{}^3c - {}^2c.{}^3b) + {}^2a({}^1c.{}^3b - {}^1b.{}^3c) + {}^3a({}^1b.{}^2c - {}^1c.{}^2b)}\,;$$

on voit donc que le numérateur de l'expression de μ, se forme du dénominateur, en y changeant a en p; on aura ensuite μ' ou μ'', en changeant dans l'expression de μ," &c.

This mode of treatment leaves nothing to be desired. It is that which is most commonly employed in the text-books of the present day.

The next point taken up is the most important in the memoir,

and requires special attention. It is introduced as "a very simple process for considerably abridging the calculation of the equation of condition between a, b, c," &c.—that is to say, the calculation of a resultant. It is, however, something of much more value than this, involving as it does a widely general expansion-theorem to which Laplace's name has been attached, but of which we have already seen special cases stated by Vandermonde. The theorem may be described as giving an expansion of a resultant in the form of an aggregate of terms each of which is a product of resultants of lower degree. Laplace's exposition is as follows:—

> "Je suppose que vous ayez deux équations,
>
> $$0 = {}^1a.\mu + {}^1b.\mu' \,; \quad 0 = {}^2a.\mu + {}^2b.\mu' \,;$$
>
> écrivez $+ab$, et donnez l'indice 1 à la première lettre, et l'indice 2 à la seconde; l'équation de condition demandée sera $+{}^1a.{}^2b - {}^1b.{}^2a = 0$.
>
> "Je suppose que vous ayez trois équations; écrivez $+ab$, combinez ce terme avec la lettre c de toutes les manières possibles, en changeant le signe de chaque terme chaque fois que c change de place, vous aurez ainsi $+abc - acb + cab$; donnez dans chaque terme l'indice 1 à la première lettre, l'indice 2 à la seconde, l'indice 3 à la troisième, et vous aurez $+ {}^1a.{}^2b.{}^3c - {}^1a.{}^2c.{}^3b + {}^1c.{}^2a.{}^3b$; cela posé, au lieu de $+{}^1a.{}^2b.{}^3c$ écrivez $+({}^1a.{}^2b - {}^1b.{}^2a).{}^3c$; au lieu de $+{}^1a.{}^2c.{}^3b$ écrivez $-({}^1a.{}^3b - {}^1b.{}^3a).{}^2c$; et au lieu de $+{}^1c.{}^2a.{}^3b$ écrivez $+({}^2a.{}^3b - {}^2b.{}^3a).{}^1c$; l'équation de condition demandée sera
>
> $$0 = ({}^1a.{}^2b - {}^1b.{}^2a).{}^3c - ({}^1a.{}^3b - {}^1b.{}^3a).{}^2c + ({}^2a.{}^3b - {}^2b.{}^3a).{}^1c.$$
>
> "Je suppose que vous ayez quatre équations, écrivez $+abc - acb + cab$, et combinez ces trois termes avec la lettre d, en observant 1° de n'admettre que les termes dans lesquels c précède d; 2° de changer de signe dans chaque terme toutes les fois que d change de place, et vous aurez
>
> $$+ abcd - acbd + acdb + cabd - cadb + cdab \,;$$
>
> donnez ensuite l'indice 1 à la première lettre, l'indice 2 à la seconde, &c., et vous aurez
>
> $$+ {}^1a.{}^2b.{}^3c.{}^4d - {}^1a.{}^2c.{}^3b.{}^4d + {}^1a.{}^2c.{}^3d.{}^4b$$
> $$+ {}^1c.{}^2a.{}^3b.{}^4d - {}^1c.{}^2a.{}^3d.{}^4b + {}^1c.{}^2d.{}^3a.{}^4b \,;$$

cela posé, au lieu de $+{}^1a.{}^2b.{}^3c.{}^4d$ écrivez

$$+({}^1a.{}^2b - {}^1b.{}^2a).({}^3c.{}^4d - {}^3d.{}^4c),$$

et ainsi des autres termes, et l'équation de condition sera

$$\begin{aligned}0 = &({}^1a.{}^2b - {}^1b.{}^2a).({}^3c.{}^4d - {}^3d.{}^4c) - ({}^1a.{}^3b - {}^1b.{}^3a).({}^2c.{}^4d - {}^2d.{}^4c)\\ &+ ({}^1a.{}^4b - {}^1b.{}^4a).({}^2c.{}^3d - {}^2d.{}^3c) + ({}^2a.{}^3b - {}^2b.{}^3a).({}^1c.{}^4d - {}^1d.{}^4c)\\ &- ({}^2a.{}^4b - {}^2b.{}^4a).({}^1c.{}^3d - {}^1d.{}^3c) + ({}^3a.{}^4b - {}^3b.{}^4a).({}^1c.{}^2d - {}^1d.{}^2c).\end{aligned}$$

"Je suppose que vous ayez cinq équations, écrivez les six termes $+abcd - acbd + \ldots$ relatifs à quatre équations, et combinez-les avec la lettre e de toutes les manières possibles, en observant de changer de signe chaque fois que e change de place; donnez ensuite l'indice 1, &c., &c., ...; au lieu du terme $+{}^1a.{}^2c.{}^3b.{}^4e.{}^5d$ écrivez $({}^1a.{}^3b - {}^1b.{}^3a).({}^2c.{}^5d - {}^2d.{}^5c).{}^4e$, &c. . . . . . .

"Lorsqu'on aura six équations, on combinera les termes $+abcde - abced +$ &c., relatifs à cinq équations avec la lettre f, en observant 1° de n'admettre que les termes dans lesquels e précède f; 2° de changer de signe lorsque f change de place: on transformera ensuite, par la règle précédente, ..."

Notwithstanding the multiplicity of instances, the rule here illustrated is not made altogether clear. This is due to two causes,—first, the linking of one case to the case before it; and, second, the want of explicit notification that the letters $b, d, f \ldots$ are combined in one way, and the intervening letters $c, e, \ldots$ in another. For the sake of additional clearness, let us see all the steps necessary in the case of the resultant of the five equations

$$a_rx_1 + b_rx_2 + c_rx_3 + d_rx_4 + e_rx_5 = 0 \qquad (r = 1, 2, 3, 4, 5),$$

and supposing, as we ought to do, that the case of four equations has not been already dealt with. These steps are—

1°. Combining b with a subject to the condition that a precede b: result—

$$ab.$$

2°. Combining c with this *in every possible way*, the sign being &c.: result—

$$abc - acb + cab.$$

3°. Combining d with each of these terms subject to the condition that c precede d: result—

$$abcd - acbd + acdb + cabd - cadb + cdab.$$

4°. Combining e with each of these terms *in every possible way:* result—

$$abcde - abced + abecd - aebcd + eabcd$$
$$- acbde + acbed - \dots\dots\dots\dots$$
$$\dots\dots\dots\dots\dots\dots\dots\dots$$

5°. Appending indices: result—

$$a_1b_2c_3d_4e_5 - a_1b_2c_3e_4d_5 + \dots\dots$$
$$\dots\dots\dots\dots\dots\dots\dots$$

6°. Changing a_mb_n into $(a_mb_n - b_ma_n)$, c_rd_s into $(c_rd_s - d_rc_s)$, &c.: result—

$$(a_1b_2 - b_1a_2)(c_3d_4 - d_3c_4)e_5 - (a_1b_2 - b_1a_2)(c_3d_5 - d_3c_5)e_4 + \dots$$

This is the required resultant in the required form.

It is of the utmost importance to notice that what is accomplished in 1°, 2°, 3°, 4° is simply (*a*) *the finding of the arrangements of* a, b, c, d, e *subject to the conditions that* a *precede* b, *and* c *precede* d, *and obtaining each arrangement with the sign which it ought to have in accordance with Cramer's rule.* The number of necessary directions might thus be reduced to three, viz., (*a*), (5), (6), in which case (1), (2), (3), (4) would take their proper places as successive steps of a methodic and expeditious way of accomplishing (*a*).

Laplace appends a demonstration of the accuracy of this development of the resultant of the nth degree, the line taken being that if the multiplications were performed the terms found would be exactly the 1.2.3.....n terms of the resultant, and would bear the signs proper to them as such.

He then goes on to deal with a rule for obtaining a like development in which as many as possible of the factors of the terms are resultants of the *third* degree.

To do so succinctly he is obliged to introduce a *notation* for resultants. On this point his words are—

> "Je désigne par (abc) la quantité
>
> $$abc - acb + cab - bac + bca - cba,$$
>
> et par (ab) la quantité $ab - ba$, et ainsi de suite; par $({}^1a.{}^2b.{}^3c)$ j'indiquerai la quantité (abc), dans les termes de laquelle on donne 1 pour indice à la première lettre, 2 à la seconde, et 3 à la troisième; par $({}^1a.{}^2b)$, je désignerai la quantité (ab) dans les termes de laquelle on donne 1 pour indice à la première lettre, et 2 à la seconde; et ainsi de suite."

We can but remark that here again he leaves little room for improvement: his symbolism is essentially that which is still in common use.

The exposition of the rule is as follows:—

> "Je suppose maintenant que vous ayez trois équations, l'équation de condition sera
>
> $$0 = ({}^1a.{}^2b.{}^3c).$$
>
> "Je suppose que vous ayez quatre équations; écrivez $+abc$, et combinez ce terme de toutes les manières possibles avec la lettre d, en observant de changer de signe lorsque d change de place, ce qui donne $+abcd - abdc + adbc - dabc$; donnez l'indice 1 à la première lettre, l'indice 2 à la seconde, &c., et vous aurez
>
> $$+ {}^1a.{}^2b.{}^3c.{}^4d - {}^1a.{}^2b.{}^3d.{}^4c + {}^1a.{}^2d.{}^3b.{}^4c - {}^1d.{}^2a.{}^3b.{}^4c;$$
>
> au lieu du terme $+{}^1a.{}^2b.{}^3c.{}^4d$, écrivez $+({}^1a.{}^2b.{}^3c).{}^4d$; au lieu de $-{}^1a.{}^2b.{}^3d.{}^4c$, écrivez $-({}^1a.{}^2b.{}^4c).{}^3d$, et ainsi de suite, et vous formerez l'équation de condition
>
> $$0 = ({}^1a.{}^2b.{}^3c).{}^4d - ({}^1a.{}^2b.{}^4c).{}^3d + ({}^1a.{}^3b.{}^4c).{}^2d - ({}^2a.{}^3b.{}^4c).{}^1d.$$
>
> "Je suppose que vous ayez cinq équations, combinez les termes $+abcd - abdc + \&c.$, relatifs à quatre équations avec la lettre e en observant 1° de n'admettre que les termes dans lesquels d précède e; 2° de changer de signe lorsque e change de place, et vous aurez
>
> $$+ abcde - abdce + abdec + \&c.$$
>
> donnez l'indice 1 à la première lettre, l'indice 2 à la seconde, &c., et vous aurez
>
> $$+ {}^1a.{}^2b.{}^3c.{}^4d.{}^5e - {}^1a.{}^2b.{}^3d.{}^4c.{}^5e + {}^1a.{}^2b.{}^3d.{}^4e.{}^5c + \&c.;$$

> ensuite, au lieu de $+{}^1a.{}^2b.{}^3c.{}^4d.{}^5e$, écrivez $+({}^1a.{}^2b.{}^3c).({}^4d.{}^5e)$; au lieu de $-{}^1a.{}^2b.{}^3d.{}^4c.{}^5e$, écrivez $-({}^1a.{}^2b.{}^4c).({}^3d.{}^5e)$, et ainsi de suite; et en égalant à zero la somme de tous ces termes, vous formerez l'équation de condition demandée.
>
> "Je suppose que vous ayez six équations, combinez les termes $+abcde-$&c., relatifs à cinq équations avec la lettre f, en observant 1° de n'admettre que les termes où e précède f; 2° de changer de signe lorsque f change de place: donnez ensuite 1 pour indice à la première lettre, ...
>
> "Si vous avez sept équations, combinez les termes $+abcdej$ $-$&c. relatifs à six équations avec la lettre g de toutes les manières possibles; pour huit équations, combinez les termes relatifs à sept avec la lettre h, en n'admettant que les termes dans lesquels g précède h, et ainsi du reste."

The really important point in all this is in regard to the manner in which the letters are brought into combination. It will be seen that the set begun with is abc, consequently a precedes b, and b precedes c throughout: then d is combined in every possible way with this: e is combined subject to the condition that d precede e; f is combined subject to the condition that e precede f: g is combined in every way possible: h is combined subject to the condition that g precede h: and so on. It would appear therefore that the letters which are to be combined in every possible way are d and *every third one afterwards*, and that each of the other letters is conditioned to be preceded by the letter which immediately precedes it in the original arrangement $abcdefghi$. . . . Condensing these directions after the manner of the former case, we should draft the rule as follows:—

(a) Find every possible arrangement of $abcdefghi$ . . . subject to the conditions that in each arrangement we must have a, b, c in their natural order; d, e, f in their natural order; g, h, i in their natural order; and so on.

(b) Prefix to each arrangement its proper sign in accordance with Cramer's rule.

(c) Append in order the indices 1, 2, 3 . . . . to the letters of each arrangement.

(d) Change $a_m b_n c_r$ into $(a_m . b_n . c_r)$, $d_s e_x f_y$ into $(d_s . e_x . f_y)$, &c.

Without saying anything as to the verification of the developments thus obtained, Laplace concludes as follows:—

> "On décomposeroit de la même manière l'équation R en termes composés de facteurs de 4, de 5, &c., dimensions."

To show how this could be effected would have been a tedious matter, if the method of exposition used in the previous cases had been followed, viz., multiplying instances with wearisome iteration of language until the laws for the combination of the letters could with tolerable certainty be guessed. On the other hand, had Laplace condensed his directions in the way we have indicated, the rule for the case in which as many as possible of the factors are of the 4th degree could have been stated as simply as that for either of the two cases he has dealt with. The only changes necessary, in fact, are in parts (1) and (4), and merely amount to writing the letters in consecutive sets of *four* instead of *two* or *three.*

Further, when the rule is condensed in this way, the problem of finding the number of terms in any one of the new developments—a problem which Laplace solves in one case by considering how many terms of the final development each such term gives rise to—is transformed into finding the number of possible arrangements referred to in part (1) of the rule. Where the highest degree of the factors of each term is 2 and the resultant which we wish to develop is of the 2nd degree (which is the case Laplace takes), the number of such arrangements is evidently $(1.2.3 \ldots . n)/(1.2)^s$, s being the highest integer in $n/2$; if the highest degree of the factors is 3, the number of arrangements is

$$\frac{1.2.3 \ldots\ldots n}{(1.2.3)^s\,(1.2)^t},$$

where s is the highest integer in $n/3$ and t the highest integer in $(n-3s)/2$; and so on.

The facts in reduction of the claim which Laplace has to the expansion-theorem now bearing his name are thus seen to be (1) that the case in which as many as possible of the factors of the terms of the expansion are of the 2nd degree had already been given by Vandermonde; (2) that Laplace did not give a statement of his rule in a form suitable for application to all possible cases, and, indeed, was not sufficiently explicit in the statement of it for the first two cases to enable one readily to see what change would be necessary in applying it to the next case. Notwithstanding these drawbacks, however, there can be no doubt that if any *one* name is to be attached to the theorem it should be that of Laplace.

The sum of his contributions may be put as follows :—

(1) A proof of the theorem regarding the effect of the transposition of two adjacent letters in any of the new functions. (XII. 2)

(2) A mode of arriving at the known solution of a set of simultaneous linear equations. (XIII. 2)

(3) The name *resultant* for the new functions. (XV.)

(4) A notation for a resultant, *e.g.* $({}^1a.{}^2b.{}^3c)$. (VII. 2)

(5) A rule for expressing a resultant as an aggregate of terms composed of factors which are themselves resultants. (XIV. 2)

(6) A mode of finding the number of terms in this aggregate. (XVI.)

LAGRANGE (1773).

[Nouvelle solution du problème du mouvement de rotation d'un corps de figure quelconque qui n'est animé par aucune force accélératrice. *Nouv. Mém. de l'Acad. Roy.* . . . . (*de Berlin*). *Ann.* 1773 (pp. 85–120).]

The position of Lagrange in regard to the advancement of the subject is quite different from that of any of the preceding mathematicians. All of those were explicitly dealing with the problem of elimination, and therefore directly with the functions afterwards known as determinants. Lagrange's work, on the other hand, consists of a number of incidentally obtained algebraical identities which we now-a-days with more or less readiness recognise as relations between functions of the kind referred to, but which unfortunately Lagrange himself did not view in this light, and consequently left behind him as isolated instances. With him x, y, z and x', y', z' and x'', y'', z'' occur primarily as co-ordinates of points in space, and not as coefficients in a triad of linear equations; so that

$$(xy'z'' + yz'x'' + zx'y'' - xz'y'' - yx'z'' - zy'x''),$$

when it does make its appearance, comes as representing six times the bulk of a triangular pyramid and not as the result of an elimination. In days when space of four dimensions was less attempted to be thought about than at present, this circumstance might possibly account for no advance being made to like identities involving four sets of four letters x, y, z, w; x', y', z', w'; &c.

In this first memoir the algebraical identities are brought together and stated at the outset as follows :—

" Lemme.

" 1. Soient neuf quantités quelconques

$$x,\ y,\ z,\ x',\ y',\ z',\ x'',\ y'',\ z''$$

je dis qu'on aura cette équation identique

$$\begin{aligned}&(xy'z''+yz'x''+zx'y''-xz'y''-yx'z''-zy'x''^2)\\ &\quad=(x^2+y^2+z^2)(x'^2+y'^2+z'^2)(x''^2+y''^2+z'')^2\\ &\qquad+2(xx'+yy'+zz')(xx''+yy''+zz'')(x'x''+y'y''+z'z'')\\ &\qquad-\ (x^2+y^2+z^2)(x'x''+y'y''+z'z'')^2\\ &\qquad-\ (x'^2+y'^2+z'^2)(xx''+yy''+zz'')^2\\ &\qquad-\ (x''^2+y''^2+z''^2)(xx'+yy'+zz')^2\ .\end{aligned}$$

" Corollaire 1.

" 2. Donc si l'on a entre les neuf quantités précédentes ces six équations

$$\begin{aligned}x^2+y^2+z^2&=a & x'x''+y'y''+z'z''&=b,\\ x'^2+y'^2+z^2&=a' & xx''+yy''+zz''&=b',\\ x''^2+y''^2+z''^2&=a'' & xx'+yy'+zz'&=b'',\end{aligned}$$

et qu'on fasse pour abréger

$$\xi=y'z''-z'y'',\quad \eta=z'x''-x'z'',\quad \zeta=x'y''-y'x'',$$

$$\beta=\surd(aa'a''+2bb'b''-ab^2-a'b'^2-a''b''^2)\ ;$$

on aura

$$x\xi+y\eta+z\zeta=\beta.$$

On aura de plus les équations identiques suivantes

$$x'\xi+y'\eta+z'\zeta=0,\qquad x''\xi+y''\eta+z''\zeta=0$$

$$\xi^2+\eta^2+\zeta^2=a'a''-b^2,$$

$$\begin{aligned}y'\zeta-z'\eta&=bx'-a'x'', & y''\zeta-z''\eta&=a''x'-bx'',\\ z'\xi-x'\zeta&=by'-a'y'', & z''\xi-x''\zeta&=a''y'-by'',\\ x'\eta-y'\xi&=bz'-a'z'', & x''\eta-y''\xi&=a''z'-bz'',\end{aligned}$$

qui sont très faciles à vérifier par le calcul.

“ Corollaire 2.

“ 3. Si on prend les trois équations

$$\begin{aligned} x\xi &+ y\eta &+ z\zeta &= \beta\,, \\ xx' &+ yy' &+ zz' &= b''\,, \\ xx'' &+ yy'' &+ zz'' &= b'\,, \end{aligned}$$

et qu'on en tire les valeurs des quantités x, y, z, on aura par les formules connues

$$x = \frac{\beta(y'z'' - z'y'') + b'(\eta z' - \zeta y') + b''(\zeta y'' - \eta z'')}{\xi(y'z'' - z'y'') + \eta(z'x'' - x'z'') + \zeta(x'y'' - y'x'')}$$

$$y = \frac{\beta(z'x'' - x'z'') + b'(\zeta x' - \xi z') + b''(\xi z'' - \zeta x'')}{\xi(y'z'' - z'y'') + \eta(z'x'' - x'z'') + \zeta(x'y'' - y'x'')}\,,$$

$$z = \frac{\beta(x'y'' - y'x'') + b'(\xi y' - \eta x') + b''(\eta x'' - \xi y'')}{\xi(y'z'' - z'y'') + \eta(z'x'' - x'z'') + \zeta(x'y'' - y'x'')}\,;$$

donc faisant les substitutions de l'Art. préc. et supposant pour abréger

$$\alpha = a'a'' - b^2$$

on aura

$$x = \frac{\beta\xi + (a''b'' - bb')x' + (a'b' - bb'')x''}{\alpha}\,,$$

$$y = \frac{\beta\eta + (a''b'' - bb')y' + (a'b' - bb'')y''}{\alpha}\,,$$

$$z = \frac{\beta\zeta + (a''b'' - bb')z' + (a'b' - bb'')z''}{\alpha}\,.\text{”}$$

In regard to the first identity here (the so-called lemma), the important and notable point is that the right-hand member is the same kind of function of the nine quantities $x^2 + y^2 + z^2$, $xx' + yy' + zz'$, $xx'' + yy'' + zz''$, $xx' + yy' + zz'$, $x'^2 + y'^2 + z'^2$, $x'x'' + y'y'' + z'z''$, $xx'' + yy'' + zz''$, $x'x'' + y'y'' + z'z''$, $x''^2 + y''^2 + z''^2$ as the left-hand member is of the nine x, y, z, x', y', z', x'', y'', z''. Indeed, without this distinguishing characteristic, the identity would have been to us of comparatively little moment. Possibly Lagrange was aware of it; but, if so, it is remarkable that he did not draw attention to the fact. It is quite true that Lagrange's identity and the modern-looking identity

$$\begin{vmatrix} x & y & z \\ x' & y' & z' \\ x'' & y'' & z'' \end{vmatrix}^2 = \begin{vmatrix} x^2 + y^2 + z^2 & xx' + yy' + zz' & xx'' + yy'' + zz'' \\ xx' + yy' + zz' & x'^2 + y'^2 + z'^2 & x'x'' + y'y'' + z'z'' \\ xx'' + yy'' + zz'' & x'x'' + y'y'' + z'z'' & x''^2 + y''^2 + z''^2 \end{vmatrix}$$

are essentially the same; but no one can deny that the latter contains on the face of it an all-important fact which is hid in the former, and which in Lagrange's time could be made known only by an additional statement in words.

The second identity

$$x'\xi + y'\eta + z'\zeta = 0$$

is a simple case of one of Vandermonde's, viz., that regarding the vanishing of his functions when two of the letters involved were the same.

The third identity

$$\xi^2 + \eta^2 + \zeta^2 = a'a'' - b^2$$

is in modern notation

$$\begin{vmatrix} y' & y'' \\ z' & z'' \end{vmatrix}^2 + \begin{vmatrix} z' & z'' \\ x' & x'' \end{vmatrix}^2 + \begin{vmatrix} x' & x'' \\ y' & y'' \end{vmatrix}^2 = \begin{vmatrix} x'^2 + y'^2 + z'^2 & x'x'' + y'y'' + z'z'' \\ x'x'' + y'y'' + z'z'' & x''^2 + y''^2 + z''^2 \end{vmatrix}$$

and is thus seen to be a simple special instance of a very important theorem afterwards discovered.

The fourth identity

$$y'\zeta - z'\eta = bx' - a'x'',$$

may be expressed in modern notation as follows:—

$$\begin{Vmatrix} y' & z' \\ |z'x''| & |x'y''| \end{Vmatrix} = \begin{vmatrix} x'x'' + y'y'' + z'z'' & x'' \\ x'^2 + y'^2 + z'^2 & x' \end{vmatrix},$$

and, quite probably, has also ere this been generalised in the like notation.

The fifth identity

$$x = \frac{\beta\xi + (a''b'' - bb')x' + (a'b' - bb'')x''}{a},$$

is not so readily transformable, the determinantal theorem which it involves being indeed completely buried. Multiplying both sides by a; then doing away with a, which seems perversely introduced "pour abréger" when no like symbol of abridgment takes the place of $a''b'' - bb'$ or of $a'b' - bb''$; and transposing, we have

$$\beta\xi = x(a'a'' - b^2) - x'(a''b'' - bb') + x''(bb'' - a'b')$$

$$= \begin{vmatrix} x & b'' & b' \\ x' & a' & b \\ x'' & b & a'' \end{vmatrix};$$

that is, finally,

$$|xy'z''| \cdot |y'z''| = \begin{vmatrix} x & xx' + yy' + zz' & xx'' + yy'' + zz'' \\ x' & x'^2 + y'^2 + z'^2 & x'x'' + y'y'' + z'z'' \\ x'' & x'x'' + y'y'' + z'z'' & x''^2 + y''^2 + z''^2 \end{vmatrix},$$

which we recognise as an instance of the multiplication-theorem on putting

$$\begin{vmatrix} x & y & z \\ x' & y' & z' \\ x'' & y'' & z'' \end{vmatrix} \times \begin{vmatrix} 1 & x' & x'' \\ 0 & y' & y'' \\ 0 & z' & z'' \end{vmatrix}$$

for the left-hand member.

LAGRANGE (1773).

[Solutions analytiques de quelques problèmes sur les pyramides triangulaires. *Nouv. Mém. de l'Acad. Roy. . . . .* (*de Berlin*) *Ann.* 1773 (pp. 149–176).]

In this memoir also there is a preparatory algebraical portion, the subject being the same as before, and the author's standpoint unchanged. Indeed the two introductions differ only in that the second is a rounding off and slight natural development of the first.

In addition to ξ, η, ζ, we have now $\xi', \eta', \zeta', \xi'', \eta'', \zeta''$ used as abbreviations for $zy'' - yz'', xz'' - zx'', \ldots\ldots$; in addition to a, we have $a', a'', \beta, \beta', \beta''$, standing for $aa'' - b'^2$, $aa' - b''^2$, $b'b'' - ab$, $bb'' - a'b'$, $bb' - a''b''$; and X, Y, Z, X', Y', ..., A, A', . . . are introduced, having the same relation to $\xi, \eta, \zeta, \xi', \eta', \ldots\ldots a, a', \ldots$ as these latter have to $x, y, z, x', y', \ldots\ldots, a, a', \ldots\ldots$ Lagrange then proceeds :—

> "3. Or en substituant les valeurs de ξ, ξ', &c., en x, x', &c., et faisant pour abréger
>
> $$\Delta = xy'z'' + yz'x'' + zx'y'' - xz'y'' - yx'z'' - zy'x'',$$
>
> on trouve
>
> $$\begin{array}{lll} X = \Delta x, & Y = \Delta y, & Z = \Delta z, \\ X' = \Delta x', & Y' = \Delta y', & Z' = \Delta z', \\ X'' = \Delta x'', & Y'' = \Delta y'', & Z'' = \Delta z'', \end{array}$$
>
> donc mettant ces valeurs dans les dernières équations ci-

dessus, on aura en vertu des six équations supposées dans l' Art. 1.

$$\begin{aligned} \mathrm{A} &= \Delta^2 a, & \mathrm{B} &= \Delta^2 b, \\ \mathrm{A}' &= \Delta^2 a', & \mathrm{B}' &= \Delta^2 b', \\ \mathrm{A}'' &= \Delta^2 a'', & \mathrm{B}'' &= \Delta^2 b'', \end{aligned}$$

et de là il est facile de tirer la valeur de Δ^2 en a, a', a'', b, &c. ; car on aura d'abord

$$\Delta^2 = \frac{\mathrm{A}}{a} = \frac{\alpha'\alpha'' - \beta^2}{a}$$

et substituant les valeurs de α', α'' et β en a, a', &c. (Art. 1).

$$\Delta^2 = aa'a'' + 2bb'b'' - ab^2 - a'b'^2 - a''b''^2 ;$$

on trouvera la même valeur de Δ^2 par les autres équations. Si on remet dans cette équation les quantités x, y, z, x', &c., on aura la même équation identique que nous avons donnée dans le Lemme ci-dessus (p. 86).

"4. Il est bon de remarquer que la valeur de Δ^2 peut aussi se mettre sous cette forme

$$\Delta^2 = \frac{a\alpha + a'\alpha' + a''\alpha'' + 2(\beta b + \beta' b' + \beta'' b'')}{3} ;$$

or si on multiplie cette équation par Δ^2 et qu'on y substitue ensuite A à la place de $\Delta^2 a$, A′ à la place de $\Delta^2 a'$ et ainsi de suite (Art. préc.) on aura

$$\Delta^4 = \frac{\mathrm{A}\alpha + \mathrm{A}'\alpha' + \mathrm{A}''\alpha'' + 2(\mathrm{B}\beta + \mathrm{B}'\beta' + \mathrm{B}''\beta'')}{3} ;$$

ou bien en mettant pour A, A′, &c., leurs valeurs en α, α', &c. (Art. 2).

$$\Delta^4 = \alpha\alpha'\alpha'' + 2\beta\beta'\beta'' - \alpha\beta^2 - \alpha'\beta'^2 - \alpha''\beta''^2 ;$$

d'où l'on voit que la quantité Δ^2 et son carré Δ^4 sont des fonctions semblables, l'une de a, a', a'', b, b', b'', l'autre de α, α', α'', β, β', β''.

"5. De plus, comme l'on a (Art. 3)

$$\begin{aligned} &xy'z'' + yz'x'' + zx'y'' - xz'y'' - yx'z'' - zy'x'' \\ &= \sqrt{(aa'a'' + 2bb'b'' - ab^2 - a'b'^2 - a''b''^2)} \quad = \Delta , \end{aligned}$$

et qu'il y a entre les quantités x, y, z, x', &c., et a, a', a'', b,

&c., les mêmes relations qu'entre les quantités ξ, η, ζ, ξ', &c., et α, α', α'', β, &c. (Art. 1), on aura donc aussi

$$\xi\eta'\zeta''+\eta\zeta'\xi''+\zeta\xi'\eta''-\xi\zeta'\eta''-\eta\xi'\zeta''-\zeta\eta'\xi''$$
$$=\sqrt{}(\alpha\alpha'\alpha''+2\beta\beta'\beta''-\alpha\beta^2-\alpha'\beta'^2-\alpha''\beta'''^2) \quad =\Delta^2.$$

Donc on aura cette équation identique et très remarquable

$$\xi\eta'\zeta''+\eta\zeta'\xi''+\zeta\xi'\eta''-\xi\zeta'\eta''-\eta\xi'\zeta''-\zeta\eta'\xi''$$
$$=(xy'z''+yz'x''+zx'y''-xz'y''-yx'z''-zy'x'')^2."$$

The remaining portion is of little importance; its main contents are four sets of nine identities each, viz. :—

1. $x\xi+x'\xi'+x''\xi''=\Delta, \qquad y\xi+y'\xi'+x''\xi''=0$, &c.
2. $x\xi+y\eta+z\zeta=\Delta, \qquad x'\xi+y'\eta+z'\zeta=0$, &c.
3. $\xi=\dfrac{\alpha x+\beta''x'+\beta'x''}{\Delta}$, &c.
4. $x=\dfrac{a\xi+b''\xi'+b'\xi''}{\Delta}$, &c.

Besides the fact that Art. 3 contains a proof of the Lemma of the previous memoir, we have to note the new identity

$$\mathrm{X}=\Delta x,$$

which in modern determinantal notation is

$$\left\|\begin{matrix}|xz''| & |yx''|\\ |zx'| & |xy'|\end{matrix}\right\| = x\,|xy'z''|,$$

—a simple special instance of the theorem regarding what is now-a-days known as "a minor of the determinant adjugate to another determinant."

The last two lines of Art. 4 by implication make it almost certain that Lagrange did not look upon

$$xy'z''+yz'x''+zx'y''-xz'y''-yx'z''-zy'x''$$
$$\text{and}\quad aa'a''+2bb'b''-ab^2-a'b'^2-a''b''^2$$

as functions of the same kind.

The new theorem in Art. 5, which Lagrange justly characterises as "very remarkable," is in modern determinantal notation

$$\left\|\begin{matrix}|y'z''| & |z'x''| & |x'y''|\\ |zy''| & |xz''| & |yx''|\\ |yz'| & |zx'| & |xy'|\end{matrix}\right\| = \begin{vmatrix}x & y & z\\ x' & y' & z'\\ x'' & y'' & z''\end{vmatrix}^2$$

—a simple instance of the theorem which gives the relation, as we now say, "between a determinant and its adjugate."

In regard to the remaining identities which we have numbered (1), (2), (3), (4), we note that (1) and (3) are not new, although (3) is here given almost in the form desiderated above (pp. 580–1); (2) involves the fact that Δ is the same function of $x, x', x'', y, y', y'', z, z', z''$, as it is of $x, y, z, x', y', z', x'', y'', z''$; and (4) may be transformed as follows:—

$$\begin{aligned} x\Delta &= a\xi + b''\xi' + b'\xi'', \\ &= \begin{vmatrix} a & y & z \\ b'' & y' & z' \\ b' & y'' & z'' \end{vmatrix}, \\ &= \begin{vmatrix} x^2 + y^2 + z^2 & y & z \\ xx' + yy' + zz' & y' & z' \\ xx'' + yy'' + zz'' & y'' & z'' \end{vmatrix}; \end{aligned}$$

so that it may be considered as another disguised instance of the multiplication-theorem, the determinant just reached being equal to

$$\begin{vmatrix} x & y & z \\ x' & y' & z' \\ x'' & y'' & z'' \end{vmatrix} \times \begin{vmatrix} x & 0 & 0 \\ y & 1 & 0 \\ z & 0 & 1 \end{vmatrix}.$$

LAGRANGE (1773).

[Recherches d'Arithmétique. *Nouv. Mém. de l'Acad. Roy. . . . (de Berlin) Ann.* 1773 (pp. 265–312).]

This is an extensive memoir on the numbers "qui peuvent être représentées par la formule $Bt^2 + Ctu + Du^2$". At p. 285 the expression

$$py^2 + 2qyz + rz^2$$

is transformed into

$$Ps^2 + 2Qsx + Rx^2$$

by putting
$$y = Ms + Nx,$$
and
$$z = ms + nx,$$
and Lagrange says—

> ". . . . je substitue dans la quantité $PR - Q^2$ les valeurs de P, Q et R, et je trouve en effaçant ce qui se détruit
>
> $$PR - Q^2 = (pr - q^2)(Mn - Nm)^2; \ . \ . \ . \ ."$$

which we at once recognise as the simplest case of the theorem connecting (as we now say) the discriminant of any quantic with the discriminant of the result of transforming the quantic by a linear substitution.

Putting now in compact form all the identities obtained from the three preceding memoirs of Lagrange, we have—

(1) $$(xy'z''+yz'x''+zx'y''-xz'y''-yx'z''-zy'x'')^2$$
$$=aa'a''+2bb'b''-ab^2-a'b'^2-a''b''^2, \qquad \text{(XVII.)}$$
where $a=x^2+y^2+z^2, \quad a'=\ldots\ldots$

(2) $\xi^2+\eta^2+\zeta^2=a'a''-b^2$, where $\xi=y'z''-z'y''$, $\eta=\ldots\ldots$ (XVIII.)

(3) $y'\zeta-z\eta=bx'-a'x''$. (XIX.)

(4) $\xi\Delta=\alpha x+\beta''x'+\beta'x''$, where $\alpha=a'a''-b^2$, $\beta''=\ldots\ldots$,
and $\Delta=xy'z''+yz'x''+zx'y''-xz'y''-yx'z''-zy'x''$. (XVII. 2)

(5) $X=\Delta x$, where $X=\eta'\zeta''-\zeta'\eta''$. (XX.)

(6) $$(xy'z''+yz'x''+zx'y''-xz'y''-yx'z''-zy'x'')^2$$
$$=\xi\eta'\zeta''+\eta\zeta'\xi''+\zeta\xi'\eta''-\xi\zeta'\eta''-\eta\xi'\zeta''-\zeta\eta'\xi''. \qquad \text{(XXI.)}$$

(7) $PR-Q^2=(pr-q^2)(Mn-Nm)^2$, (XXII.)
if $$p(Ms+Nx)^2+2q(Ms+Nx)(ms+nx)+r(ms+nx)^2$$
$$=Ps^2+2Qsx+Rx^2 \text{ identically.}$$

BÉZOUT (1779).

[Théorie Génerale des Equations Algébriques, §§ 195–223, pp. 171–187; §§ 252–270, pp. 208–223. Paris.]

In his extensive treatise on algebraical equations Bézout was bound, as a matter of course, to take up the question of elimination; and, as he had dealt with the subject in a separate memoir in 1764, one might not unreasonably expect to find the treatise giving merely a reproduction of the contents of the memoir in a form suited to a didactic work. Such, however, is far from being the case. He merely mentions the necessary references to the work of Cramer, himself, Vandermonde, and Laplace; and then adds—

> “Mais lorsqu'il a été question d'appliquer ces différentes méthodes au problème de l'élimination, envisagé dans toute

> son étendue, je me suis bientôt apperçu qu'ils laissoient tous encore beaucoup à desirer du côté de la pratique."

His main objection to the said methods is that when one has to deal with a set of equations of no great generality, with coefficients, it may be, expressed in figures—

> "Il faut construire ces formules dans toute la generalité dont les équations sont susceptibles, et faire par conséquent le même travail que si les équations avoient toute cette généralité.
>
> . . . .
>
> (197). Au lieu donc de nous proposer pour but seulement, de donner des formules générales d'élimination dans les équations du premier degré, nous nous proposons de donner une règle qui soit indifféremment et également applicable aux équations prises dans toute leur généralité, et aux équations considerées avec les simplifications qu'elles pourront offrir: une règle dont la marche soit la même pour les unes que pour les autres, mais qui ne fasse calculer que ce qui est absolument indispensable pour avoir la valeur des inconnues que l'on cherche: une règle qui s'applique indifféremment aux équations numériques et aux équations littérales, sans obliger de recourir à aucune formule. Telle est, si je ne me trompe, la règle suivante.
>
> "*Règle générale pour calculer, toutes à la fois, ou séparément, les valeurs des inconnues dans les équations du premier degré, soit littérales soit numériques.*
>
> "(198). Soient u, x, y, z, &c., des inconnues dont le nombre soit n, ainsi que celui des équations.
>
> "Soient a, b, c, d, &c., les coëfficiens respectifs de ces inconnues dans la première équation.
>
> "a', b', c', d', &c., les coëfficiens des mêmes inconnues dans la seconde équation.
>
> "a'', b'', c'', d'', &c., les coëfficiens des mêmes inconnues dans la troisième équation: et ainsi de suite.
>
> "Supposez tacitement que le terme tout connu de chaque équation soit affecté aussi d'une inconnue que je représente par t.
>
> "Formez le produit $uxyzt$ de toutes ces inconnues écrites

dans tel ordre que vous voudrez d'abord; mais cet ordre une fois admis, conservez-le jusqu'à la fin de l'opération.

" Echangez successivement, chaque inconnue, contre son coëfficient dans la première équation, en observant de changer le signe à chaque échange pair: ce résultat sera, ce que j'appelle, une *première ligne.*

" Echangez dans cette *première ligne*, chaque inconnue, contre son coëfficient dans la seconde équation, en observant, comme ci-devant, de changer le signe à chaque échange pair: et vous aurez une *seconde ligne.*

" Echangez dans cette *seconde ligne*, chaque inconnue, contre son coëfficient dans la troisième équation, en observant de changer le signe à chaque échange pair: et vous aurez une *troisième ligne.*

" Continuez de la même manière jusqu'à la dernière équation inclusivement; et la dernière *ligne* que vous obtiendrez, vous donnera les valeurs des inconnues de la manière suivante.

" Chaque inconnue aura pour valeur une fraction dont le numérateur sera le coëfficient de cette même inconnue dans la dernière ou n^e *ligne*, et qui aura constamment pour dénominateur le coëfficient que l'inconnue introduite t se trouvera avoir dans cette même n^e *ligne.*"

The application of this very curious rule is illustrated by a considerable number of varied examples, of which we select the second—

" (200). Soient les trois équations suivantes

$$\begin{aligned} ax + by + cz + d &= 0\,, \\ a'x + b'y + c'z + d' &= 0\,, \\ a''x + b''y + c''z + d'' &= 0\,. \end{aligned}$$

" Je les écris ainsi

$$\begin{aligned} ax + by + cz + dt &= 0\,, \\ a'x + b'y + c'z + d't &= 0\,, \\ a''x + b''y + c''z + d''t &= 0\,. \end{aligned}$$

Je forme le produit $xyzt$.

Je change successivement x en a, y en b, z en c, t en d, et observant la règle des signes, j'ai pour première ligne

$$ayzt - bxzt + cxyt - dxyz\,.$$

Je change successivement x en a', y en b', z en c', t en d', et observant la règle des signes, j'ai pour seconde ligne

$$\begin{gathered}(ab'-a'b)zt-(ac'-a'c)yt+(ad'-a'd)yz\\+(bc'-b'c)xt-(bd'-b'd)xz+(cd'-c'd)xy.\end{gathered}$$

Je change successivement x en a'', y en b'', z en c'', t en d'', et observant la règle des signes j'ai pour troisième ligne

$$\begin{aligned}&[(ab'-a'b)c''-(ac'-a'c)b''+(bc'-b'c)a'']t\\-&[(ab'-a'b)d''-(ad'-a'd)b''+(bd'-b'd)a'']z\\+&[(ac'-a'c)d''-(ad'-a'd)c''+(cd'-c'd)a'']y\\-&[(bc'-b'c)d''-(bd'-b'd)c''+(cd'-c'd)b'']x.\end{aligned}$$

D'où (198) je tire

$$x=\frac{-[(bc'-b'c)d''-(bd'-b'd)c''+(cd'-c'd)b'']}{(ab'-a'b)c''-(ac'-a'c)b''+(bc'-b'c)a''},$$

$$y=\frac{+[(ac'-a'c)d''-(ad'-a'd)c''+(cd'-c'd)a'']}{(ab'-a'b)c''-(ac'-a'c)b''+(bc'-b'c)a''},$$

$$z=\frac{-[(ab'-a'b)d''-(ad'-a'd)b''+(bd'-b'd)a'']}{(ab'-a'b)c''-(ac'-a'c)b''+(bc'-b'c)a''}.\text{''}$$

Among the other examples are included (1) one in which the coefficients in the set of equations are given in figures; (2) one in which some of the coefficients are zero; (3) one showing the simplification possible when the value of only one unknown is wanted; (4) one showing the signification of the vanishing of one of the "*lignes*"; (5) one showing the signification of the absence of one of the unknowns from the last "*ligne*"; and (6) one or two concerned with the allied problem of elimination.

Bézout nowhere gives any reason for his rule; it is used throughout as a pure rule-of-thumb: its effectiveness being manifest, he leaves on the reader the full burden of its arbitrariness. The unreal product $xyzt$ at the very outset must have been a sore puzzle to students, and none the less so because of the certainty which many of them must have felt that a real entity underlay it.

To throw light upon the process, let us compare the above solution of a set of three linear equations with the following solution, which from one point of view may be looked upon as an improvement on the ordinary determinantal modes of solution as presented to modern readers.

The set of equations being

$$\left.\begin{array}{l} ax\ +by\ +cx\ +d\ =0 \\ a'x\ +b'y\ +c'x\ +d'\ =0 \\ a''x+b''y+c''x+d''=0 \end{array}\right\}$$

we know that the numerator of the values of x, y, z, and the common denominator are

$$-\begin{vmatrix} b & c & d \\ b' & c' & d' \\ b'' & c'' & d'' \end{vmatrix}, \quad +\begin{vmatrix} a & c & d \\ a' & c' & d' \\ a'' & c'' & d'' \end{vmatrix}, \quad -\begin{vmatrix} a & b & d \\ a' & b' & d' \\ a'' & b'' & d'' \end{vmatrix}, \quad +\begin{vmatrix} a & b & c \\ a' & b' & c' \\ a'' & b'' & c'' \end{vmatrix}.$$

They are therefore the coefficients of x, y, z, t in the determinant

$$\begin{vmatrix} a & b & c & d \\ a' & b' & c' & d' \\ a'' & b'' & c'' & d'' \\ x & y & z & t \end{vmatrix}, \text{ or } \Delta \text{ say.}$$

Thus the problem of solving the set of equations is transformed into finding the development of this determinant. In doing so let us use $[xyz]$ to stand for the determinant of which x, y, z is the last row, and whose other rows are the two rows immediately above x, y, z in Δ: similarly let $[zt]$ stand for the determinant of which z, t is the last row, and its other row the row c'', d'' immediately above z, t in Δ; and so on in all possible cases, including even $[xyzt]$, which of course is Δ itself.

Then clearly we have

$$[xyzt]=a[yzt]-b[xzt]+c[xyt]-d[xyz] \quad . \ . \ . \ . \quad (1)$$

Developing in the same way the four determinants here on the right side, we have as our next step

$$\begin{aligned}[xyzt]= \ & a(b'[zt]-c'[yt]+d'[yz]) \\ & -b(a'[zt]-c'[xt]+d'[xz]) \\ & +c(a'[yt]-b'[xt]+d'[xy]) \\ & -d(a'[yz]-b'[xz]+c'[xy]), \\ = \ & (ab'-a'b)[zt]-(ac'-a'c)[yt]+(ad'-a'd)[yz] \\ & +(bc'-b'c)[xt]-(bd'-b'd)[xz]+(cd'-c'd)[xy].\end{aligned}$$

Again, developing the six determinants $[zt]$, $[yt]$, ... in the same way, and rearranging the terms, we have finally

$$\begin{aligned}[xyzt] = {} & \{(ab'-a'b)c''-(ac'-a'c)b''+(bc'-b'c)a''\}t \\ & -\{(ab'-a'b)d''-(ad'-a'd)b''+(bd'-b'd)a''\}z \\ & +\{(ac'-a'c)d''-(ad'-a'd)c''+(cd'-c'd)a''\}y \\ & -\{(bc'-b'c)d''-(bd'-b'd)c''+(cd'-c'd)b''\}x.\end{aligned}$$

But the coefficients of x, y, z, t in $[x\,y\,z\,t]$ were seen on starting to be the numerators and the common denominator of the values of x, y, z in the given set of equations: hence

$$x = \frac{-\{(bc'-b'c)d''-(bd'-b'd)c''+(cd'-c'd)b''\}}{\{(ab'-a'b)c''-(ac'-a'c)b''+(bc'-b'c)a''\}}$$

$$y = \;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.$$

$$z = \;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.$$

2. The Formation of the Germinal Layers in Teleostei. By George Brook, F.L.S., Lecturer on Comparative Embryology in the University of Edinburgh.

(*Abstract.*)

The majority of recent investigations have concluded that the thickening of the rim of the blastoderm, and the new layer which is derived from it, is brought about by a true invagination of the *segmented* blastoderm. On the other hand, Klein and Kupffer hold that the newly formed layer is not derived from the segmented blastoderm (archiblast), but is mainly produced from an *unsegmented* portion of the blastoderm (parablast), which is intermediate in position between the archiblast and the yolk.

A little over a year ago I was led to conclude, from a study of the development of *Trachinus*, that the so-called "invaginated layer," the primitive hypoblast, was in great part, if not entirely, built up from the margin inwards of cells budded off from the parablast. More recently I have studied the question in other forms, particularly the herring, and am now enabled to bring forward more important evidence as to the origin and morphological significance of the parablast, and also of the part which this layer plays in the economy of the embryo.

The first two furrows in the herring are vertical in direction, and at right angles to one another. They are not pushed down to the base of the germinal mound, but cease some distance before reaching the yolk. Four segmentation spheres are thus formed, which are imperfectly separated from the lower pole of the ovum. The third furrow takes an equatorial direction, and simply completes the bases of the first four segmentation spheres. The upper portion goes on segmenting, and constitutes the archiblast. The lower pole consists of a central mass of yolk, around which is a layer of protoplasm, which constitutes the parablast. With the formation of the first equatorial furrow we have a division of the ovum into an animal and a vegetative pole, as in the amphibian ovum.

The archiblast represents the animal pole, and the parablast, with its enclosed yolk, the vegetative. The segmentation cavity appears between them. The primitive hypoblast is formed from the vegetative pole by a budding off of cells from the margin of the parablast, as has already been described. Cell formation takes place in the parablast of the herring ovum earlier than I have observed it in other forms, and two batches of cells are added to the animal pole by this means before the pseudo-invagination begins. In the herring the archiblast, together with the cells which have been derived from the parablast prior to the formation of the segmentation cavity, gives rise to the ectoderm. The primitive hypoblast, which is almost entirely derived from the vegetative pole, gives rise to the mesoderm and notochord, and the true entoderm remains as a single row of cells in connection with the parablast.

If this account should prove to be correct, several interesting relations will be established between Teleostei and other forms. The primitive hypoblast, as I have here described it, is precisely homologous with that of Amphioxus, and both give rise to mesoderm, notochord, and entoderm.

Again the derivatives of the animal and vegetative pole in the herring are practically the same as in the frog. In Teleosteans, however, the vegetative pole consists at first of one cell instead of four, this arrangement being brought about by the greater bulk and different arrangement of the yolk.

Quite recently Dr Ruckert, who has investigated the question in Elasmobranchs, has come to very similar conclusions.

3. Account of a Preliminary Experiment on the Thermal Conductivity of Ice. By A. Crichton Mitchell. Communicated by Professor Tait. (Plate XXI.)

The following paper is an account of a preliminary experiment performed in the Edinburgh University Physical Laboratory, with the view of determining the thermal conductivity of ice.

The method employed was that of Angström, which consists in heating and cooling alternately, to a definite amount and for definite periods of time, one end of a block or bar of the substance whose conductivity is required. The thermal changes produced in this manner are transmitted throughout the substance in a species of wave. Taking two points in the substance in a line perpendicular to the surface which is being heated and cooled, these changes are produced until the temperature indications at the two successive points have become periodic. The conductivity can then be calculated from the ratio of the amplitudes of the temperature indications per unit length along the line joining the two points, and from the interval in time between the maximum temperatures per unit length.

Obviously, the ranges of temperature we must work with in the case of ice lie below 0° cent.; and accordingly, the periodic cooling and heating were effected by successive applications of a freezing mixture and exposure to the surrounding air, which was throughout the experiment never above 1° cent. To carry this out, a tinned iron box, with open top, had two thermo-electric junctions of iron-German silver—inserted through india-rubber corks, fitted into circular holes, in opposite sides of the box; one hole being so much above the other as to separate the junctions by a distance of 1·27 centimetres from each other, in a line perpendicular to the bottom of the box. The cube was then filled with water, previously boiled to expel dissolved air, and placed on the roof of the College for forty-eight hours during frost. The ice formed in this manner, with the thermo-electric junctions in its interior, was free from cracks and air particles, and was evidently quite homogeneous.

It was at first attempted to cool and heat the surface of the ice by simply laying on it a freezing mixture of snow and salt for a definite period of time, and subsequently flooding the surface of the

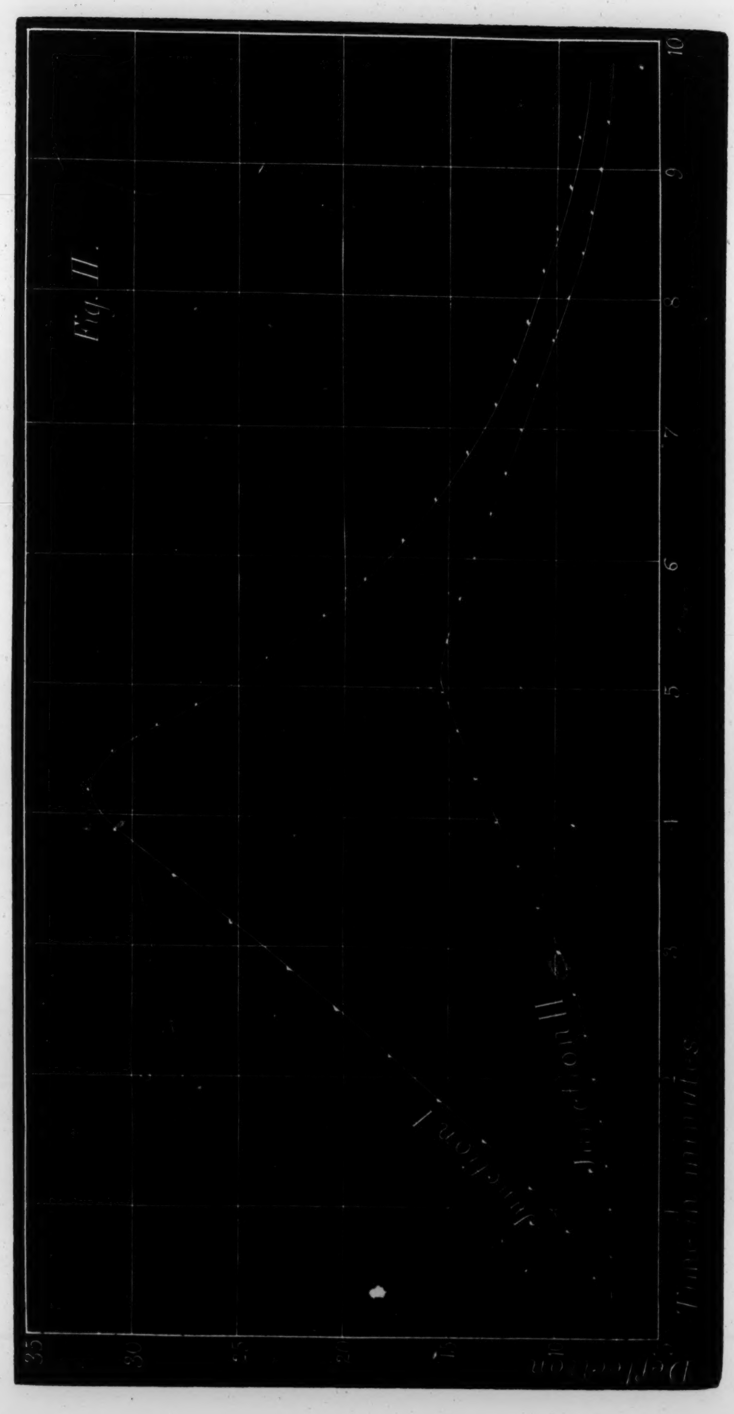
Junction I
Junction II
Time in minutes
Deflection
35
30
25
20
15
10
1
2
3
4
5
6
7
8
9
10

Fig. II.

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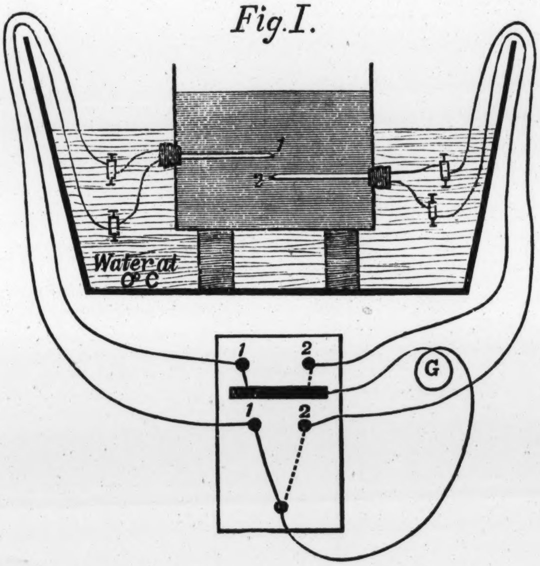
ice with ice-cold water for the same time. But the first of these two operations was found to melt the ice on its surface; while during the second, the ice-cold water froze on the surface of the previously cooled ice. To obviate this difficulty, a thin sheet of tinfoil was placed on the suface of the ice; on this was laid for five minutes the freezing mixture of pounded ice and salt; and during the next five minutes the surface of the ice was simply exposed to the air. This alternation of cooling and heating was then regularly carried on until the thermal changes at the thermo-electric junctions were observed to be periodic; after which readings were taken at equal intervals of time, viz., 20 seconds.

Fig. I.



The metal-box, containing the cube of ice, was placed in a bath of ice and water, which provided a steady temperature for one of the junctions in each of the two thermo-electric circuits. By means of a commutator, one junction (in the ice) at a time was placed in circuit with a Thomson's dead-beat galvanometer (resistance 1·5 ohms at 9°·4 cent.); the circuit was so changed as to include the two junctions alternately. In this manner it was found possible to take readings every ten seconds.

It was found difficult to attain a strictly periodic change in the temperature at each of the junctions. This may be due partly to the fact that freezing mixtures themselves undergo sudden changes of temperature, and partly to the circumstance that the completeness of contact between the intervening sheet of tinfoil and the surface of the ice may not have been the same during two successive periods.

J_1	J_2	J_1	J_2	J_1	J_2	J_1	J_2
27·7		42·7		40·8		3·00	
	27·4		29·6		34·4		28·9
28·3		45·1		38·8		30·0	
	27·4		30·5		33·7		28·8
29·6		48·1		37·1		29·4	
	27·3		31·5		33·1		28·2
31·5		50·6		35·7		29·2	
	27·3		32·4		32·4		27·85
33·7		51·9		33·5		28·8	
	27·5		33·6		31·2		27·6
35·8		50·7		32·7		28·65	
	27·9		34·4		30·5		27·3
38·0		47·1		31·8		28·5	
	28·5		35·1		30·0		27·1
40·2		43·5		31·1		28·5	
	29·0		34·8		29·4		27·05

Galvanometer zero, 29·0.

Detailed above are a set of readings taken during one period—one of the few in which the changes were practically periodic. The junctions are numbered in order, beginning with that nearer the surface. The interval between two readings of the same junction is 20 seconds, and they were taken alternately.

Plotting these readings (which express differences of temperature) against time, a curve is obtained, the equation to which is

$$\begin{aligned} f(\theta) = A_0 + A_1 \cos\theta + A_2 \cos 2\,\theta + A_3 \cos 3\theta + \ldots . \\ + B_1 \sin\theta + B_2 \sin 2\,\theta + B_3 \sin 3\,\theta + \ldots . \end{aligned}$$

where θ is an angle which increases in proportion to the time, and where A_0, A_1, A_2, B_1, B_2, . . . &c., are unknown coefficients. By taking six values of $f(\theta)$ we may determine the coefficients A_0, A_1, A_2, A_3, B_1, B_2, B_3. Subtracting from each value of $f(\theta)$, A_0, the mean value of the function, we get

$$\begin{aligned} \eta = A_1 \cos\theta + \ldots \\ + B_1 \sin\theta + \ldots \end{aligned}$$ Then putting $P_1 = \sqrt{A_1^2 + B_1^2}$,

this may be written

$$\eta = P_1 \left(\frac{A}{P_1} \cos\theta + \frac{B_1}{P_1} \sin\theta\right) + \ldots . . \&c.$$

Evidently $\frac{A_1}{P_1}$ and $\frac{B_1}{P_1}$ are respectively the cosine and sine of some angle. Let it be α. Then we have

$$\eta = P_1 \cos(\theta - \alpha_1) + P_2 \cos 2(\theta - \alpha_2) + \dots\dots$$

Having determined P_1 and α_1 for the first junction, P_1' and α_1' are determined in the same way for the second.

Then $$\frac{P_1'}{P_1} = \epsilon^{-px} \text{ and } \alpha_1' - \alpha_1 = qx,$$

where x is the distance between the junctions. Having calculated p and q, the conductivity κ may be found by the formula

$$\frac{\kappa}{c\rho} pq = \frac{\pi}{T},$$

where c = specific heat,
ρ = density,
T = length of period.

Detailed below is the full working out for the period during which the readings above given were taken :—

Junction I. *corrected for* A_0

$$\begin{aligned}
\eta_0 &= 7{\cdot}7 &&= -9{\cdot}01 \\
\eta_1 &= 15{\cdot}8 &&= -0{\cdot}91 \\
\eta_2 &= 28{\cdot}15 &&= +11{\cdot}44 \\
\eta_3 &= 23{\cdot}5 &&= +6{\cdot}89 \\
\eta_4 &= 14{\cdot}5 &&= -2{\cdot}21 \\
\eta_5 &= 10{\cdot}6 &&= -6{\cdot}11 \\
6 \; & \underline{\overline{|100{\cdot}25}} \\
\therefore A_0 &= 16{\cdot}71
\end{aligned}$$

$$\begin{aligned}
6\,A_1' &= 2(\eta_0 - \eta_3) + (\eta_1 - \eta_2) - (\eta_4 - \eta_5) \\
&= -31{\cdot}8 - 12{\cdot}35 - 3{\cdot}9 = -48{\cdot}05 \\
\therefore A_1 &= -8{\cdot}01 \\
2\sqrt{3}B_1 &= (\eta_1 - \eta_5) + (\eta_2 - \eta_4) \\
&= 5{\cdot}2 + 13{\cdot}65 = 18{\cdot}85 \\
\therefore \sqrt{3}B_1 &= 9{\cdot}43 \quad \therefore B_1 - 5{\cdot}48
\end{aligned}$$

$$\therefore P_1 = 9{\cdot}706 \text{ and } \alpha_1 = 145^\circ\, 37'$$

Junction II. *as above.*

$$\begin{aligned}
\eta_0 &= 7{\cdot}6 &&= -2{\cdot}85 \\
\eta_1 &= 7{\cdot}7 &&= -2{\cdot}75 \\
\eta_2 &= 11{\cdot}0 &&= +0{\cdot}55 \\
\eta_3 &= 15{\cdot}05 &&= +5{\cdot}60 \\
\eta_4 &= 12{\cdot}2 &&= +1{\cdot}75 \\
\eta_5 &= 9{\cdot}15 &&= -1{\cdot}30 \\
6 \; & \underline{\overline{|62{\cdot}7}} \\
\therefore A_0 &= 10{\cdot}45
\end{aligned}$$

$$\begin{aligned}
6\,A_1' &= -23{\cdot}15 \\
\therefore A_1 &= -3{\cdot}86 \\
2\sqrt{3}B_1' &= -2{\cdot}65 \\
\therefore B_1' &= -{\cdot}768
\end{aligned}$$

$$\therefore P_1' = 3{\cdot}936. \quad \alpha_1' = 191^\circ\, 19'.$$

$$\log \frac{P_1}{P_1'} = px + \log \epsilon. \quad \therefore p = {\cdot}708. \quad \alpha_1' - \alpha_1 = qx \quad \therefore q = {\cdot}625$$

$$\frac{\kappa}{c\rho} pq = \frac{\pi}{T} \qquad \therefore \kappa = {\cdot}005.$$

(in C. G. S. units).

This experiment was performed on Saturday, January 9, 1886, and the period for which readings have been given, and from which the determination $\kappa = \cdot 005$ has been obtained, occurred between 11 A.M. and mid-day of that date. It was attempted again in the evening, beginning at 5.30 P.M. Shortly after 6 P.M. the galvanometer was seen to give irregular deflections, even when not included in one of the thermo-electric circuits. These deflections continued to increase to such an extent that the directing magnet had to be shifted to keep the light-spot on the scale. These phenomena were found to be due to the disturbed state of the earth's magnetism at the time, for from 7.30 P.M. till 8.30 P.M. on that evening a brilliant auroral display took place. Some readings taken at 8.20 P.M. showed a deflection of the needle through 2° 15′—this even when the disturbance was passing off.

The ratio of the intensity of the magnetic field due to the earth *alone*, to that due to the earth *and* magnet has since been tested, and found to be $\frac{1735}{1800}$, the arrangement being such that, superposed on the field due to the earth alone was another, almost double but opposite.

In all probability the readings, taken immediately before this disturbance was noticed, were affected by it; for, on taking the readings from the last two periods in the evening's work (about 6.15 P.M.), and calculating the conductivity therefrom, I find it to be ·011 and ·010 respectively.

As already stated, the period from which the determination $\kappa = \cdot 005$ was obtained occurred about mid-day, and there is no reason to believe that the readings during that period were vitiated by the cause which rendered those taken in the evening untrustworthy.

PRIVATE BUSINESS.

The Right Hon. J. H. A. Macdonald, Professor Frederick O. Bower, Mr Robert Irvine, Dr G. Sims Woodhead, Mr Arthur W. Hare, Mr George Fosbery Lyster, Dr D. Noel Paton, the Rev. George Laing, and Mr William Macdonald were balloted for, and declared duly elected.

Monday, 15th February 1886.

ROBERT GRAY, Esq., Vice-President, in the Chair.

The following Communications were read:—

1. Chemical Affinity and Solution. By W. Durham, Esq.

2. On the Reproductive Elements in *Myxine glutinosa*, L.

By J. T. Cunningham, B.A.

(*Abstract.*)

The mature Ovum of Myxine.—The earliest description of the mature ovum is that given by Allen Thomson, in the article "Ovum" in Todd's *Cyclopædia of Anatomy and Physiology*, vol. v., published in the year 1859. He says:—"I have found that in the *Myxine glutinosa* the globular yolk is enclosed in a horny capsule of similar consistence and structure (to that of Elasmobranchs), but of a simple elongated ellipsoidal shape; and in place of four terminal angular tubes there are a number of trumpet-shaped tubular processes projecting from the middle of the two ends, which probably serve the same purpose as the differently shaped appendages of the ova of the shark and skate." He gives a figure in woodcut of the ovum, and of one of the threads magnified. The figures are correct with this exception: in the representation of the entire ovum the yolk is represented in outline as though seen through the envelope, and is globular, like the yolk of Elasmobranchs. In the Myxine ovum the yolk fills the cavity of the ellipsoidal envelope. It is not true that the terminal processes are tubular. I have examined them, and found them solid, and, as far as can be seen with a low power, homogeneous. I have not been able to ascertain from what specimen or specimens the description of Thomson is taken, but there is a single specimen of the ripe ovum in Professor Turner's museum which may have been examined by Thomson. This specimen was brought to the museum in the time of Professor Goodsir, but it is not possible now to ascertain whether it was dredged from the sea-bottom or taken from a pregnant female.

In 1863 Professor Steenstrup,* of Copenhagen, published a description of some ripe ova found in a female Myxine sent to the Copenhagen Museum in the month of September 1862. He also compares the envelope of the ovum with its polar threads to the capsule of the deposited ovum in Elasmobranchs. The ova were lying at the edge of the ovary, to which they were only connected by the entangling of the threads. In the woodcut to Steenstrup's paper one of the eggs has the polar portion of the capsule at one end detached like an operculum.

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* *Oversigt Dansk. Vidensk. Forhandlenger*, 1863.

I have recently, by cutting sections of very large ovarian eggs of Myxine, ascertained that the envelope of the deposited egg is developed entirely within the follicle. It is usual to distinguish primary egg membranes as chorion or vitelline membrane, according as they are developed by secretion from the follicular epithelium, or from the ovum. I am inclined to think that in this sense the envelope of the Myxine egg is a chorion, but I have not yet completely examined the question of the development of the membrane in all details. The important point is that the envelope is a primary egg membrane, and that the threads are solid processes from it. In sections of the polar region of ovarian eggs about 19 mm. in length, the ovum is surrounded by three distinct layers, corresponding to those universally found in vertebrate ovaries. The most external is the connective tissue layer, which is of considerable thickness, and is continuous with the thin mesoarium. Next to this is the follicular epithelium, composed of several layers of elongated cells, arranged with their long axes perpendicular to the surface of the ovum. Next is the egg-membrane, which is homogeneous, and forms the sole envelope of the deposited ovum. At one pole is found a cellular process projecting from the follicular epithelium, and penetrating through the chorion. Thus a canal is formed in the latter, constituting the micropyle. But this aperture is not at this stage open internally; the micropyle penetrates nearly, but not quite, to the inner surface of the chorion.

In the vitellus, close beneath the inner end of the micropyle, is seen the germinal vesicle, which is globular, large, and distinct. In my preparations nucleoli could not be made out, but from its appearance the vesicle did not seem to have undergone much change from its original condition.

In older eggs, about 21 mm. long, the beginning of the formation of the polar threads may be seen. In sections of the polar region of such eggs, the plane of the sections being parallel to the longer axis of the egg, deep pits are seen in the follicular epithelium. These pits are expanded at the bottom, and reach nearly to the connective tissue layer of the follicle, only a thin layer of epithelium separating the cavity of the pit from the inner surface of the connective tissue. Corresponding to these pits are seen processes from the chorion; there can be no doubt that in the fresh living condition the processes entirely fill up the pits. The chorion itself is much thicker than in the stage previously described, the processes are directly continuous with it, and like it are, at least as seen with ordinary magnifying powers, homogeneous. The processes are quite solid, and the inner surface of the chorion turned towards the vitellus is as even as in the earlier stage. The micropyle is seen again at one pole of the egg, but its condition is a little different. It is open externally, but closed by a thin portion of the chorion internally. It still contains a cellular thread, but this thread is shrunken in size, and has now no connection with the follicular epithelium, the surface of which is unbroken above the micropyle.

Without further evidence, I concluded that the further development of the egg consisted in the thickening of the chorion and the elongation of the threads, these processes being due apparently to the secretory activity of the follicular epithelium. The probability of this being the case is shown by the fact that both epithelium and chorion are much thicker at the poles than at the equatorial region of the ovarian eggs. I had no doubt that the follicle burst and allowed the escape of the egg enclosed in its chorion with the threads at the poles. But I was unable from the above facts to ascertain exactly how the follicle opened. I had seen bodies in the ovary of some specimens of Myxine which looked like aborted eggs, and had surmised that these were corpora lutea, but they were too degenerate to afford any evidence of the exact manner in which the ovum had escaped. But in December and January I found a female specimen of Myxine in the ovary of which were no large eggs, but a number of collapsed follicles, from which the ripe ova had evidently escaped quite recently; examining these follicles I found that each possessed a slit-like aperture at one end, and thus the question of the escape of the ovum with its threads is elucidated.

I have also recently obtained a female specimen of Myxine in which all the large eggs in the ovary had a slight projection at each end, and in these knobs the ends of the threads could be seen through the connective tissue. In these eggs the polar threads were half way or more towards their complete development.

In January I obtained two or three other specimens in which the eggs were nearly as ripe, the projections formed by the development of the threads being visible, but not quite so prominent. In sections of the ripest eggs above described it is seen that the follicular epithelium at the poles of the egg has become greatly thickened, while the connective tissue layer has become thinner. The threads are contained in deep cylindrical sacs formed by the epithelium; the distal ends of these sacs project deeply into the connective tissue layer, which is very thin over their terminations. The epithelium is thick all round these sacs, and it is quite evident that the threads and the layer from which they project are formed by the epithelium. The micropyle in these ripest eggs is in the same condition as before, and is still closed by a thin layer of chorion at its inner end. I have not yet found the germinal vesicle in the eggs at this stage. The question whether a vitelline membrane exists internal to the chorion I have not yet decided.

Development of the Ova.—The ovary of Myxine differs from that of other vertebrata, or craniata, in consisting of an extremely thin fold instead of a somewhat solid mass. It is impossible to define the boundary between ovary and mesoarium. The mesoarium extends the whole length of the body on the right side only (in two specimens I have found it developed on the left side only), and is attached at the angle between the mesentery and intestine. The eggs originate at the free edge of the ovary, which is only slightly thickened, and as they grow large pass inwards, the largest eggs being always nearest to the attached border of the mesoarium. There is no transition in thickness between the membranous mesoarium and the thick connective-tissue layer of the follicle, but the former passes abruptly into the latter. The line of attachment of the mesoarium passes round the longest circumference of the ellipsoidal follicle. When the eggs are large they stretch the mesoarium and hang down beyond the edge of the ovary, but the relations pointed out are never altered, not even when the egg escapes. The very young eggs are transparent and spherical, and show a distinct germinal vesicle in the fresh condition. As the eggs elongate the germinal vesicle passes to one end, but the yolk soon develops and conceals that structure from view.

Male Elements of Reproduction.—Professor Steenstrup, in the paper above mentioned, says that no one seems to have seen a male Myxine. This is not quite correct, for Johannes Müller, in the last volume of his *Verglerichende Anatomie der Myxinoiden*, published in 1845, describes the testis of Myxine in a somewhat vague manner, but in such a way that, from my own experience, I have no doubt that he really had a young male specimen under his hands. The males are very rare; out of all the specimens which I have examined only ten have been males, and these are all immature. It is extremely difficult to distinguish between a male and a very immature female; in most cases I have only been able to do so by microscopic examination of the generative organ. The testis is similar in general structure to the ovary, but the free border is slightly more thickened than in the latter organ, and has the gelatinous appearance characteristic of testicular tissue. When examined with a low power the thickened edge is seen to contain a number of more or less spherical capsules, and these on compression are seen to be filled with rounded polygonal cells. When the capsule is burst the cells escape, and are then seen to be spherical, extremely transparent, and provided with a large nucleus. It is easy to conclude that these cells give rise to spermatozoa. The spermatozoa of Myxine hitherto have never been seen or described.

Hermaphrodite Specimens.—After identifying the immature males, I found that in nearly every specimen in which the eggs were very immature the posterior end of the sexual organ was similar in structure to the testis. The testicular portion is usually much smaller than the ovarian, and occupies about 2 inches of the posterior end, but sometimes extends further forward. These hermaphrodites are almost as common as the females. The most curious fact about the hermaphrodite forms is, that in two of them I have found the early stages of the development of spermatozoa, although in the males I never found anything but the spherical cells above described. In one of these two specimens I found spermatozoa within the testicular capsules, but these spermatozoa were few in number and not in motion. I have not yet been able to work out completely the process of spermatogenesis, but it seems that the cells of the testicular capsules send off processes, each of which becomes a spermatozoon. Only two or three of these processes are formed at a time, and the spermatoblast seems to develop a number of spermatozoa in succession, instead of, as in nearly all other cases, simultaneously. The spermatozoa have a pear-shaped head, and a long tail, which is attached to the obtuse end of the head.

I venture to suggest the hypothesis in explanation of the above facts, that the males are nothing more than supplementary males, and that the Myxine in its young state is nearly always, if not always, hermaphrodite. It is evident that, as the spermatozoa are developing in the hermaphrodites while the eggs are still very small, self-fertilisation is impossible; but it seems extremely probable that the hermaphrodites while young act as males, and that most eggs are fertilised by them. The females with large eggs are never hermaphrodite, and it may be inferred that as the eggs develop the testicular portion of the reproductive organ disappears. There may be some females which never possess a testicular portion, as there are perfect males, but of this I cannot be certain. If the above hypothesis be correct, it would account for the extreme rarity of perfect males. Another fact, which is partially accounted for by my investigations, is the extreme rarity of female specimens with ripe eggs. The eggs are certainly deposited in winter, and eggs approaching maturity are not to be found in the summer months. But even now the specimens with eggs nearly ripe bear a very small proportion to the total number obtained, and it is probable that the animals, when just preparing to shed their sexual products, cease to feed, and therefore cannot be allured by bait to either hook or trap, which are the only known methods of capturing them. One thing deserves to be mentioned with reference to the male elements. In the hermaphrodites, in which spermatozoa were being developed, the total size of the testicular portion of the generative organ was extremely small, compared to that of the testis in all other vertebrates, and the quantity of milt shed, unless a sudden development takes place at the last moment, must be extremely small. This fact seems to indicate that fertilisation does not take place in the open water, and it is possible that the eggs and milt are shed in the mud in which the animal lives.

3. The Life-History of the Micro-organisms associated with Variola and Vaccinia. An Abstract of Results obtained from a Study of Small-Pox and Vaccination in the Surgical Laboratory of the University of Edinburgh. By J. B. Buist, M.D., F.R.C.P., Edinburgh.

> "Go to the quality of the active principle; abstract it from the material, and contemplate it by itself. Then determine the time; how long, at furthest, this thing, of this particular quality, can naturally subsist."—*Marc. Aurel.*, Book ix. sect. 25.

ORIGIN OF INQUIRY.

The following investigations were undertaken with the view of ascertaining the nature and cause of opacity in vaccine lymph. According to the authorities on the subject, clear fresh lymph is a perfect material for vaccination, while opaque lymph, fresh or stored, is an imperfect material, but hitherto the nature of the difference between them has not been explained satisfactorily. It has been maintained by many that opaque lymph is a satisfactory material for the purpose of vaccination, but this view is not endorsed by the National Vaccine Establishment. In April, last year, Professor Chiene suggested to me that the cause of opacity in lymph was to be found in a "germ," and he cordially assented to my request for assistance in making the necessary cultivations in order to determine its nature. The cultivations detailed were made by his assistant Mr Hare, whose intimate acquaintance with modern methods of bacteriological research has saved much time and labour. The responsibility of the observations recorded rests upon me entirely.

A preliminary microsopic examination of clear and opaque vaccine lymph led to no definite result, as the appearances observed could not be explained, It was therefore decided to examine—

I. Empty Commercial Vaccine Tubes for "germs."
II. Cultivations of Vaccine Lymph.
    a, By the naked-eye; *b*, by the microscope.
III. Cultivations of Variolous Lymph.
    a, By the naked-eye; *b*, by the microscope.
IV. Clear and Opaque Vaccine and Variolous Lymph.
V. The Results of Experimental Vaccination.

I. EXAMINATION OF EMPTY COMMERCIAL VACCINE TUBES.

The problem to be solved was whether such tubes contain germinal matter. To determine this, two series of experiments were undertaken.

1. Sealed commercial tubes were introduced with aseptic precautions into beakers containing sterile nutrient fluid. Continued sterility of the fluid was proved by subsequent incubation at 35° C. The tubes were then broken with aseptic precautions. Subsequent incubation showed that more than half of the beakers containing broken tubes became cloudy.

Where the sealed tubes were left unbroken, but where the fluid had merely been stirred with a sterile glass rod, one out of six only became cloudy.

2. Commercial and sterilised vaccine tubes were charged with sterile and non-sterile fluid, with and without spray, in imitation of the conditions under which vaccine lymph might be stored. The result showed that sterility of the tube and of the fluid and the use of the spray were necessary to prevent the occurrence of opacity. Where these conditions were absent opacity occurred in every tube.

The fact that a large proportion of the beakers containing broken tubes showed no change in the fluid, led to the conclusion that the amount of germinal matter in commercial tubes was very small. Besides, the amount of opacity in the fluid in the second series of experiments was very much less than that observed in vaccine lymph. It was therefore concluded, that the contents of the tubes had very little to do with the production of opacity in vaccine lymph. This conclusion is concurred in by Mr Farn, of the National Vaccine Establishment.

II. EXAMINATION OF CULTIVATIONS OF VACCINE LYMPH.

All the cultivations were made by Mr Hare with aseptic precautions. The material was transferred directly from the vesicles to the cultivating medium. All the successful cultivations were got from clear lymph. None were got from opaque lymph. The appearances were noted in each cultivation on three separate occasions.

1. Table *showing the Naked-Eye Appearances of Cultivations of Vaccine Lymph.*

No.	Name of Tube.	4th Day.	8th Day.	Subsequently.
1	I. A, agar.	White.	Faint yellow.	Yellow.
2	I. B, ,,	,,	,,	,,
3	II. A, ,,	,,	Yellowish green.	Strong yellow.
4	II. B, ,,	,,	,,	,,
5	I. ,,	No report.	No report.	Grey and orange.
6	II. ,,	,,	,,	White and yellow.
7	I. A, jelly.	No reaction.	,,	,,
8	I. B, ,,	No report.	White, yellow, orange.	Orange.
9	II. A, ,,	,,	White and yellow.	Yellow.
10	II. B, ,,	,,	,,	Yellow "cocoons."
11	II. ,,	,,	No report.	,,
12	I. A′, agar.	Secondary	cultivation.	Dull orange
13	II. B′, ,,	,,	,,	White & yel. "cocoons."
14	I. B′, jelly.	,,	,,	Bright orange.
15	II. A′, ,,	,,	,,	White and yellow.
16	I. jelly, yellow.	Secondary, 11th day.	Orange.	Orange.
17	I. jelly, white.	,,	,,	,,
18	III. B, agar, 477.	White.	Grey and dull orange.	Dull orange.
19	III. A, ,,	Faint white.	White in cloud.	White.
20	III. A, jelly, 477.	No reaction.	No reaction.	No reaction.
21	III. B, ,,	,,	,,	,,
22	IV. A, agar, 480.	From opaque	lymph.	,,
23	IV. B, ,,	,,	,,	,,
24	IV. A, jelly, 480.	,,	,,	,,
25	IV. B, ,,	,,	,,	,,
26	V. A, agar, 478.	White.	White.	White "cocoons."
27	V. B, ,,	,,	,,	,,
28	V. A, jelly, 478.	No report.	No report.	White & yel. "cocoons."
29	V. B, ,,	,,	,,	,,
30	VI. A, agar, 475.	Greyish blue.	Greyish white.	Thick white growth.
31	VI. B, ,,	,,	,,	,,
32	VI. A, jelly, 475.	White "cocoons."	White.	Jelly liquefied.
33	VI. B, ,,	,,	,,	Yellow.
34	VII. A, jelly, 479.	,,	White "cocoons."	White.
35	VII. B, agar, 479.	White.	Dull orange.	Dull orange and brown.
36	VIII. A, agar, 483.	White & dull orange.	White.	Strong white growth.
37	VIII. B, agar, 483.	,,	Dull orange.	Dull orange.
38	IX. A, jelly, 481.	White.	White and yellow.	White and yellow.
39	IX. B, agar, 481.	,,	White.	Yellow.
40	X. A, agar, 485.	,,	,,	White and yellow.
41	X. B, ,,	,,	White and dull ochre.	,,
42	XI. agar, 487.	,,	White.	White, yellow, & brown.

An analysis of the preceding table shows that in primary cultivations the colour of the growth, with scarcely any exception, was white on the fourth day. Primary cultivations in Koch's jelly were invariably white in the form of "cocoons." On the eighth day yellow and orange colour appeared in a certain proportion, but the prevailing colour was still white. After this the principal colours were white and yellow, only one cultivation being of an orange colour. We had thus three distinct growths, white, yellow and orange, as described by other observers. Secondary cultivations were made from certain of the tubes which had been opened for microscopic examination. In only one case out of six were the same colours reproduced. Change of colour in the growth was thus observed to take place both in primary and secondary cultivations. It is difficult to explain how this can take place on the supposition that we have three different organisms to deal with.

2. *Histological Examination of Vaccine Cultivations.*

A. Orange cultivation showed swarms of minute spherical "micrococci" without definite arrangement.

B. White cultivations showed single, double, and triple micrococci, and in one case a sarcina form. The micrococci were larger in cultivations which had been incubated.

C. Yellow cultivations showed dumb-bell and sarcina form micrococci. Here also the incubated cultivations showed larger torula-form micrococci.

We could not explain these different appearances. It was supposed that we had three different organisms, any one of which might be the immediate cause of vaccinia. Other preparations of vaccine lymph, clear and opaque, were now made.

In clear lymph certain minute badly stained spherical bodies were recognised similar to those of the orange vaccine cultivation, but I am indebted to Dr Francis Troup for the first clear demonstration of them in vaccine lymph. I then saw that the "micrococci" in clear lymph and orange "vaccine" are identical. Opaque lymph after being kept some time showed large spherical transparent bodies like oil drops, but their nature was a mystery. Even after examining vaccine cultivations, we were as far off as ever from being able to explain the nature and cause of opacity in lymph. I therefore suggested that we should cultivate variola.

III. EXAMINATION OF CULTIVATIONS OF VARIOLOUS LYMPH.

As there were no cases of small-pox in Edinburgh suitable for our purpose, it became necessary to visit the Hospital Ships at Purfleet, on the Thames, to obtain variolous cultivations. I have to acknowledge the readiness with which Dr Birdwood placed the material at his disposal at our service, and also the able assistance given by Messrs Bott and Clatworthy, the resident physicians.

The variolous cultivations may be conveniently divided into three classes:—

1. Where no reaction or growth occurred.
2. Where growth occurred without liquefaction.
3. Where growth was accompanied by liquefaction.

1. The first class is of value as showing the care with which contamination of the media was prevented. The third class was excluded from present comparison with vaccine cultivations, by the occurrence of liquefaction of the media. The second class may fairly be compared with vaccine cultivations. But we had found that the most definite and easily recognised form of growth was the "cocoon" in Koch's jelly. Our series is therefore reduced to cultivations of variola, showing this distinct mode of growth. Nos. 11, 12, and 39 only showed a large definite "cocoon" growth without liquefaction. These were selected as parallel, and probably identical, with vaccine cultivations of the same appearance. The questions to be decided were, as to their histological appearances, physiological action, and contagiousness.

2. *Histological Investigation of Variolous Cultivations.*

A. White "variola" showed single, double, and triple micrococci, identical with white "vaccine."

B. Clear variolous lymph showed minute spherical bodies, similar if not identical with those of clear vaccine lymph and orange "vaccine."

C. Opaque variolous lymph showed large torula-looking "micrococci," as well as smaller forms.

IV. EXAMINATION OF CLEAR AND OPAQUE VACCINE AND VARIOLOUS LYMPH.

On comparing clear and opaque variolous lymph with clear and opaque vaccine lymph, the micro-organisms in each appeared identical. My observations are thus in accordance with the description given by Cohn, quoted by Burdon Sanderson in his Report on the Intimate Pathology of Contagion. Cohn was unable to say whether there was any connection between the bodies in clear and opaque lymph; but Zopf, in his work *Die Spaltpilze*, 1885, states that the opacity "consists in cell rows and masses resulting from the continuous division of the cocci." He recognises no other modes of development.

TABLE *showing the General Naked-Eye Appearances of Cultivations of Variolous Lymph in Solid Media.*

Source.	No.	Name of Tube.	8th day.	13th day.	Result.
Early pustular or pustular.	1	Aa, jelly.	No reaction.	No reaction.	No reaction.
	2	Ab, ,,	,,	,,	,,
	3	Ac, ,,	,,	,,	,,
	4	Ad, ,,	,,	,,	,,
	5	Ae, agar.	,,	,,	,,
	6	Af, ,,	,,	,,	,,
	7	Ag, ,,	,,	,,	Trace of white.
	8	Ah, ,,	,,	,,	,,
	9	Ai, serum.	White riband.	White.	White.
	10	Aj, ,,	No reaction.	Trace of white.	Trace of white.
Blood, papulo-vesicular.	11	Ba, jelly.	White "cocoon."	White "cocoon."	Larger, pure white.
	12	Bb, ,,	,,	,,	,,
	13	Bc, ,,	No reaction.	No reaction.	No reaction.
	14	Bd, agar.	White cloud.	White cloud.	White cloud.
	15	Be, ,,	,,	,,	,,
	16	Bf, ,,	No reaction.	No reaction.	No reaction.
	17	Bg, serum.	,,	,,	,,
	18	Bh, ,,	,,	,,	,,
Vesicular.	19	Ca, agar.	White zone.	White zone.	White zone.
	20	Cb, ,,	White cloud.	White cloud.	White cloud.
	21	Cc, ,,	White zone.	White zone.	White zone.
	22	Cd, serum.	White cloud.	White cloud.	White cloud.
	23	Ce, ,,	,,	,,	,,
Papulo-vesicular.	24	Da, jelly.	White "cocoons."	White "cocoons"	White "cocoons."
	25	Db, ,,	Liquefaction, and	white p.p. at bottom of	liquid.
	26	Dc, ,,	No reaction.	No reaction.	No reaction.
	27	Dd, agar.	White.	White.	Faint white.
	28	De, ,,	No reaction.	,,	White.
	29	Df, ,,	,,	No reaction.	No reaction.
	30	Dg, serum.	,,	,,	,,
	31	Dh, ,,	White riband.	White.	White.
Vesicular.	32	Ea, jelly.	No reaction.	No reaction.	No reaction.
	33	Eb, ,,	White "cocoon."	White "cocoon."	White.
	34	Ec, ,,	,,	,,	Liquefying.
	35	Ed, ,,	,,	Large white, liquefying	with white p.p.
	36	Ee, ,,	,,	White.	White.
	37	Ef, ,,	"Cocoons" and	liquefying globe, with	white p.p.
	38	Eg, ,,	,,	,,	,,
	39	Eh, ,,	White cocoon.	Larger white.	Pure white "cocoon."
	40	Ei, ,,	No reaction.	Small "cocoon."	White.
	41	Ej, ,,	White "cocoon."	Globe of liquefaction,	with orange p.p.
	42	Ek, ,,	,,	White.	Faint white.
	43	El, ,,	"Cocoons."	Globe of liquefaction,	with yellow p.p.
	44	Em, agar.	White cloud.	White cloud.	White cloud.
	45	En, ,,	White zone.	Thick white zone.	White.
	46	Eo, ,,	Liquefying.	Liquefying.	Liquefying.
	47	Ep, ,,	White cloud.	White.	White.
	48	Eq, ,,	,,	,,	,,
	49	Er, ,,	Liquefying?	Liquefying?	Solid white.
	50	Es, ,,	White cloud.	White cloud.	Wh., with orange patch.
	51	Et, ,,	No reaction.	No reaction.	No reaction.
	52	Eu, serum.	White.	White.	White riband.
	53	Ev, ,,	No reaction.	No reaction.	No reaction.

TABLE—*Cultivations of Variolous Lymph*—continued.

Source.	No.	Name of Tube.	8th day.	13th day.	Result.
Papulo-vesicular.	54	Fa, jelly.	Faint White.	White.	Faint white.
	55	Fb, „	White cocoons.	„	„
	56	Fc, „	„	„	White "cocoons."
	57	Fd, „	No reaction.	No reaction.	No reaction.
	58	Fe, „	White.	White.	Liquef. and white p.p.
	59	Ff, „	„	„	Faint growth.
	60	Fg, agar.	„	Thick white.	White cloud.
	61	Fh, „	„	„	„
	62	Fi, „	„	„	„
	63	Fj, „	No reaction.	No reaction.	No reaction.
	64	Fk, „	„	„	„
	65	Fl, serum.	White riband.	White riband.	White riband.
	66	Fm, „	„	„	„
Bullæ.	67	Ga, jelly.	Funnel-shaped	liquefaction, with	white p.p.
	68	Gb, „	„	„	„
	69	Gc, „	„	„	„
	70	Gd, „	„	„	„
	71	Ge, agar.	Orange or ochre.	Cocoons solid.	Ochre cocoons.
	72	Gf, „	„	„	„
	73	Gg, „	„	„	„
	74	Gh, „	White cloud.	White cloud.	White cloud.
	75	Gi, „	White and orange.	White and ochre.	White and orange.

V. EXPERIMENTAL VACCINATION.

We divide the experiments into four series—

I. Vaccination of Calves.
II. Vaccination of Guinea Pigs.
III. Vaccination of Monkeys.
IV. Contagion Experiment.

Ten experiments were performed, as is shown by the following table:—

No.	Vaccine Material.	Vaccinifer.	Result.
I.	White, yellow, and orange "vaccine."	Calf.	Pustular eruption on head.
II.	Opaque variolous lymph.	Calf.	Pustular eruption on back.
III.	Vesicular variolous lymph.	Calf I.	Protected by "vaccine" I.
IV.	Pustular variolous lymph.	Calf II.	Protected by variolation II.
V.	Orange "vaccine."	Guinea pig.	Local scab.
VI.	White "vaccine."	Guinea pig.	No local result. Constitutional?
VII.	Yellow "vaccine."	Guinea pig.	No local result. Constitutional?
VIII.	Clear vaccine lymph.	Monkey.	Four typical vaccine vesicles.
IX.	White variolous "cocoon."	Monkey.	Eight pocks. No local result.
X.	Modified variola IX.	Monkey.	Not contagious.

An analysis of this table shows that orange "vaccine" produces a local scab in a guinea pig, and that clear vaccine lymph produces typical vaccination in monkeys. Vaccine cultivations and opaque variolous lymph produce an eruptive fever in calves, when inoculated. No local result is produced. White variolous cultivation produces a mild variolous eruption in a monkey, which does not appear to be contagious to another monkey. No local result is produced at the points of inoculation. Protection is afforded from the poison of variola by previous variolation and vaccination with "vaccine" cultivations. White and yellow "vaccine" produce no local result in guinea pigs. They probably produce a mild constitutional result. The experiments appear to show that different vaccine materials possess different degrees of potency.

CLASSIFICATION OF MICRO-ORGANISMS.

In attempting to trace the life-history of the vaccine and variolous organisms from the preceding observations we are confronted by a preliminary difficulty. At present the fission-fungi are undergoing the most minute investigation, and the result has been attempts to improve upon the classifications of Cohn and Naegeli. Naegeli held that the bacteria were allied to yeasts, and should therefore be included in the class of fungi. The difficulty in accepting this view arose from the fact that fungi were supposed to be destitute of colouring matter. Owing to this, Cohn placed them among algæ, but the tendency now is to amalgamate the colourless fungi (bacteria) and the colour-producing algæ (bacteria) into one group, the thallophytes (Sachs). The latest classifications of Flügge, 1883, and Zopf, 1885, appear to me to be unsatisfactory, and I prefer to follow the classification of Cohn into four tribes, including six genera. He believes that the form or shape characteristic of each tribe is adhered to throughout the life of the organism. Thus, a micrococcus cannot be transformed into a bacterium or bacillus, but retains its spherical shape. We have only to do at present with Cohn's first group, the Sphærobacteria, comprising one genus, the micrococci. While, however, Cohn has settled the main lines upon which the classification of Schizomycetes should be based, he admits that his classification of the genera into species is defective. This is due to deficient knowledge of their physiological action and modes of reproduction. Thus, micrococci having the same appearance may have different effects. Cohn divides his genus, Micrococcus, into three physiological species—(1) Chromogenic, (2) Zymogenic, (3) Pathogenic. He places the Micrococcus Vaccinæ among the pathogenic micrococci, but the record of the cultivations just detailed shows that the vaccine organism is also chromogenic, so that it might equally well be classed among these coloured micrococci. At the same time, he admits that differences in arrangement and size are unreliable data upon which to found the classification of species. A necessity, therefore, arises for the acceptance to some extent of the theory of pleomorphism (Tulasne), *i.e.*, that the same plant can occur under two or more forms, as well with respect to the organs of vegetation as to those of reproduction. Applying this theory to the bacteria, we find that a single species may show various forms in the course of its life-cycle. This has been shown by the researches of De Bary, Zopf, Dallinger, Douglas Cunningham, Ewart and Geddes, and others.

The propagation of fungi takes place asexually in three ways—1 By free cell formation (asci, thecæ, spore-pouches). 2. Constriction (basidians). 3. By cell-fission or gemmation. Spores are the chief means of the spread of fungi (Wagner).

Surrounding media modify the form and mode of fructification of fungi, some increasing and others diminishing spore-formation, or we may have the fungus dividing into yeast forms. Schwann, Pasteur, and others consider the yeast fungi as organisms *sui generis*, arising in fermentable liquids from their own specific germs.

Hallier, Hoffmann, and others consider that they are only conditions, especially of mould fungi occurring in fermentable liquids, particularly the spore forms, which fructify in the atmosphere in other forms. They originate likewise from spores or from yeast cells themselves when they reach a liquid. Hoffmann thinks that the genera of Schizomycetes, described by Ehrenberg, Pasteur, &c., pass into one another, peculiarities which are to be held as characteristic of the species, and which change in the course of development according to the change in the external conditions of life (Wagner). As we have seen, Cohn maintains that such genera are distinct throughout life. Species may show various sizes and different pigments besides differing in their effects. Zopf is a warm supporter of the theory of pleomorphism. Crookshank (*Bacteriology*, 1886), who adopts Zopf's classification, says that in classifying species of bacteria, we must take into account—1. Microscopical appearances in various nutrient media. 2. Character of their colonies under a low power, in plate cultivations. 3. Microscopical appearances of the organisms themselves. 4. Their physiological action. These desiderata, however, scarcely satisfy the requirements of the case, and I beg leave to add, 5. The study of their modes of development and reproduction in each species.

In this way we may be able to make out the life-history of an organism. Recent observations by Lister, Neelsen, Zopf, Van Tieghem, Klein, and Hauser, are said (Crookshank) to show that the orders of Cohn pass into one another. It is due to Lister to say, however, that he retracted his first opinion with regard to the bacillus of black milk, and expressed a doubt as to whether he had not got a mixture of organisms. If this were true, of course, Cohn's classification of genera would fall to the ground. Cohn's strong point is that he opposes change of shape in genera. He admits differences in size and arrangement and colour, as well as physiological action, in the different species of each genus. The truth appears to be, that while the various genera retain their globular, oval, rod-shaped or spiral form throughout life, the different species described as cocci, rods, threads, and spirals may be merely stages of growth of a single organism. At the same time these various forms may produce different physiological effects. We find so-called "cocci" described as growing into torulæ or rods. But we find that rods are spore-bearing, so that it appears rather a misnomer to call such spores cocci. Should they not be called spores of bacilli? It is evident that a gap has to be filled up between the higher torula-form and the spore. The researches of Dr Douglas Cunningham upon the micro-organisms found in the intestinal canal show that they have an active and a resting stage. In suitable media, the "zoospores" multiply till the material for their growth is exhausted, when the medium is acid, and the micro-organisms cannot develop further. When transferred to an alkaline medium they at once become active.

The mode of reproduction of bacterium termo has been well described by the Rev. W. H. Dallinger, in a remarkable paper communicated to the Royal Society of London in 1878 by Professor

Huxley. He finds that the bacterium grows to an ovoid body, of very delicate nature, which bursts and gradually collapses, scattering very minute spores. By continuous observation, he has seen the spore grow again into the ovoid body, which was seen to burst again as before. Mr Dallinger worked with very high powers, × 3000. Similar observations with regard to the life-history of bacteria have been made by Professor Cossar Ewart, but he does not emphasise the resting stage. Mr P. Geddes points out that this occurs in many instances in the cycle of cell-life.

TABLE *showing the Histological Appearances of the Vaccine Organism in Solid and Fluid Media*, (× 700).

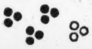
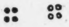
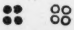
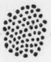
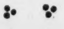
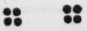
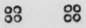
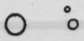
No.	Medium.	Material.	Forms.	Corresponding Forms.
1	Fluid.	Clear lymph.	· ·	Burdon Sanderson's "microzyme." Cohn's spheroidal corpuscle.
2	,,	,,	• •	Cohn's simple corpuscle or M. Vaccinæ.
3	Solid.	White in jelly.	••	Cohn's "pairs of corpuscles."
4	,,	White in agar.		Cohn's "groups" or "clumps."
5	,,	Yellow in jelly.		Cohn's "groups resembling sarcinæ."
6	,,	Yellow in agar.		Cohn's "larger aggregations" or "clumps."
7	Fluid.	Opaque lymph.	○	Burdon Sanderson's "semifluid material" or "oil-drops." Cohn's "refractive cells resembling oil-drops."
8	Solid.	Orange, 8th day.		Burdon Sanderson's "microzyme." Cohn's "minute spheroidal corpuscle."

TABLE *showing Appearances of Variolous Organisn in Solid and Fluid Media, (after Cohn)*, (× 700).

No.	Medium.	Material.	Forms.	Corresponding Forms.
1	Fluid.	Clear lymph.	• ⁖	Cohn's minute sphere.
2	,,	,,	• ⁖	Cohn's simple corpuscle.
3	Solid.	White in jelly.	••	Cohn's pairs of corpuscles.
4	,,	,,		Cohn's "groups" or "clumps."
5	Fluid.	Opaque lymph.		Cohn's "groups resembling sarcinæ."
6	,,	,,		Cohn's "larger aggregations."
7	,,	,,		Cohn's "refractive cells resembling oil-drops."
8				

LIFE HISTORY OF THE VACCINE ORGANISM.

In tracing this I shall follow the method recommended above by Crookshank, as my observations fall naturally under his headings.

1. *Macroscopical Appearances of Vaccine Cultivations.*—We found that the best medium for making observations was Koch's jelly. Here we obtained three definite coloured growths. The colours on the surface of the jelly were white, yellow, and orange. Below the surface they were white and yellow. No orange colour appeared below the surface. The shape of the colonies was oval or like a cocoon. Sometimes these were aggregated together, and irregular on the surface. They grow from inoculations of clear lymph only.

2. The "colonies" have been well described by Cohn, in 1872, in preserved lymph. He saw the minute spherical corpuscle of fresh clear lymph succeeded by "simple corpuscles, double corpuscles, rows and heaps or masses of corpuscles." He saw the corpuscles become larger until at last the lymph contained separate large globular homogeneous transparent bodies like oil-drops. I can confirm these observations as correct, and I agree with Zopf (1885) in thinking that they can only proceed from continuous division and growth of the organisms in clear lymph. Opacity of lymph is produced in this way. I observed, however, that the large torula-looking bodies disappeared from fresh preparations within twelve hours, and next day both stained and unstained preparations showed large masses, which had apparently grown from them by budding. I think that this appearance is produced by the bursting of the globules, but I have not seen this take place. I think that these large cells contain spores, and that they are developed from the organisms in clear lymph. If these bodies originated in accidental contamination of the lymph, it is difficult to see why they should be of constant occurrence in stored lymph, and not only so, but also in opaque lymph within the vesicles. Then they did not appear in the other nutrient fluid used to imitate vaccine lymph in the tube experiments. The stages of growth between the embryonic and the mature form have been accurately traced by Cohn, both in vaccine and variolous lymph. The larger forms are never found in fresh clear lymph.

With regard to the colonies in solid media, the first appearance is white, even when the growth is of some size. The change of colour takes place gradually, and it remains yellow. Spore formation, as shown by the bright orange colour, is seldom seen in solid media. I am unable to explain this, except that the environment is unfavourable to the production of spores, while it is favourable to the ordinary vegetative growth. Then, when secondary cultivations were made, the same colour was only once reproduced. If change occurred, it was in the direction of more distinctness in the colour, apparently showing that the change of medium was more favourable for spore production. It would be interesting to know whether coloured growths of vaccine lymph are alkaline or acid in reaction. If the orange growth is composed of spores, it should correspond to the alkaline clear lymph. At the same time, the white and yellow should show an acid reaction similar to that given by opaque lymph. We may suppose the yellow colour to be produced by a mixture of vegetative and spore growth, *i.e.*, white and orange.

3. *Microscopic Appearances of the Organisms.*—My observations show that the size of the organisms increases with the age of the cultivations. Incubation has also some effect, and one medium may be more suitable than another for its growth. Thus in Koch's jelly the micrococci were smaller than those in agar after incubation. As the material was the same, this could only be explained by more rapid growth. Thus we find that the same organism derived from the same source, presents different appearances according to the environment and other favourable circumstances. It does not follow, however, that the same forms will produce the same effects. Nor does it follow that different-sized organisms will not produce similar physiological effects. The truth seems to be that the larger organism is the more powerful. The embryonic form is milder in action.

4. *Physiological Action.*—The res~~ults~~ of experiments, although few in number, appear to show that the ~~embryo~~nic form is milder in action than the more mature form of organism. Thus clear vaccine lymph and orange vaccine, both containing minute organisms, produced only a local result. Opaque variolous lymph and white and yellow vaccine cultivations and white variolous cultivations, containing larger organisms not found in clear lymph, produce an

eruptive fever. They appear to be more powerful in action. If the organisms do not grow from the minute spheres in clear lymph, where do they come from? Are there several kinds of minute spheres in clear lymph? Even if so, according to the opposite view, they could not change their size and arrangement by cultivation only.

5. *Mode of Development and Reproduction.*—My observations appear to confirm Cohn's opinion that genera of bacteria retain their shape throughout their life-history. Micrococci remain micrococci, but they may present variations in size, arrangement, and colour, and their physiological action may become more powerful. Definite production of bright colours appears to be associated with spore-formation. The large transparent delicate torula found in fluid media, represents the organism in its resting stage, as described by Dallinger and Douglas Cunningham. The active stage commences when this mature organism is transplanted to a suitable medium. Then spore production immediately becomes complete, and a fresh cycle of growth commences. I cannot say where these spore-bearing cells originate, but it is possible that they come from yeasts or moulds.

SUMMARY OF RESULTS.

The following conclusions appear to be warranted by the results of the inquiry just detailed. Bacteriologists are still at variance as to whether bacteria spring from higher forms, and grow into them again. So far as the globular or sphæro-bacteria are concerned, the only mode of reproduction at present recognised is fission occurring in one or more directions. Different morphological appearances and colours are held to be distinctive of different species of micrococci, but it appears probable that a careful study of the physiological action and mode of reproduction is required before a satisfactory classification can be obtained. Continuous observation of each organism from its embryonic to its mature form is necessary. Cultivation of the organisms, in both solid and fluid media, also appears to be imperative. It is also necessary to distinguish between local and constitutional results of experimental inoculation with cultivations.

We conclude, then,

1. That commercial vaccine tubes contain germinal matter, but in small amount, and that it assists in the production of opacity in lymph.
2. Clear vaccine lymph, clear variolous lymph, and orange

"vaccine" contain probably, not micrococci, but the spores of micrococci. Inoculation with these materials produces local irritation by the growth of the "spore" into a more mature form of organism.

3. Opaque vaccine lymph and opaque variolous lymph contain larger, more refractive bodies of various sizes, which are probably developed from the spores in clear lymph. They increase in size and number in proportion to the length of time the lymph has been kept. After a few months large torulæ crowd the field in microscopic preparations. Such lymph has an acid reaction, and when inoculated it produces either violent local action, *i.e.*, accelerated vaccination, or we may have no local action, but secondary eruption in some other part of the body. This is probably due to rapid spore-production by the torulæ. The spores are probably absorbed into the blood, and rapidly grow there, until they are arrested in the capillaries, where they continue to grow and set up irritation ending in the production of vesicles or pustules. From these secondary foci may again be produced. This would account for the appearance of periodic secondary eruptions after vaccination. The growth from the spore to the torula explains the occurrence of opacity in lymph. Drying the lymph prevents this growth, and also deterioration of the true vaccine material.

4. White, yellow, and dull ochre or orange cultivation of vaccine lymph in solid media, appear to represent stages of growth between the spore and the torula. They correspond to the refractive corpuscles and cells described by Cohn in vaccine lymph. When inoculated, they produce in calves eruptive fever. They do not produce a local result.

5. White variolous cultivations in solid media appear to represent stages of growth between the spore in clear variolous lymph and the torula in opaque variolous material. When inoculated, a mild eruptive fever was produced in a monkey. This was not contagious to another monkey. The organisms correspond to the refractive corpuscles of Cohn. Cultivations of variola and vaccinia in solid media appear to produce mild eruptive fever. They do not produce a local result.

6. Inoculation with vaccine and variolous cultivations protects from a subsequent attack of variola in calves.

7. The best or perfect material for vaccination appears to be that which contains spores only. This is probably due to the small quantity of the virus, and to its slower rate of increase.

8. Inferior or imperfect materials for vaccination are more powerful in their action. Thus opaque vaccine and variolous lymph not unfrequently produced eruptions, with a modified imperfect local effect. Cultivation of clear lymph in solid media appears to increase its potency, as shown by its production of an eruptive fever in a calf. Cultivation of vesicular variolous lymph in solid media does not appear to differ in potency from vaccine cultivations, as shown by the effect of inoculation on a monkey. The action of the material containing spores is probably milder.

9. Vaccination probably protects from small-pox, by producing a mild form of fermentation in the blood. The process probably takes place slowly, as the ferment is growing in unsuitable soil.

10. When the spores or mature forms are inhaled they probably grow in the air-passages with extreme rapidity, and thus get into the blood, where they multiply during the incubative period.

11. My observations appear to show that what is called "attenuation of a virus" may be explained by spore-production. Are not the perfect vaccine materials for infective diseases to be found in the spores of the micro-organisms which are their exciting causes?

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4. On the probable Heats of Formation of Zinc-Copper Alloys. By A. P. Laurie, Esq.

5. On the Mean Free Paths in a mixture of Two Systems of Spheres. By Professor Tait.

6. On Two Shrunk Human Heads from South America. By Professor Duns, D.D.

The Indians, to whom the preparation of these heads is traced, are scattered over a wide tract of country on the eastern side of the Andes. They are known under the general designation Macas, the name of the region they inhabit—a designation which includes the Jivaros, Pastazas, Mendes, Tumbas, and more than twenty other tribes. Early brought into contact with the first Spanish invaders, they suffered much at their hands. Many references to them occur in the history of the Spanish settlers. Their appearance, social habits, and superstitious practices have much prominence given to them in the recent literature of South American travel. At present I limit my remarks mainly to the specimens of "shrunk" or reduced heads now before us. They are from several points of view remarkable, and raise several questions of great ethnological interest, but trustworthy references to them are comparatively few. Indeed, the only sources of information worth mentioning are a communication by Mr William Bollaert, F.R.G.S., to the Ethnological Society of London, 1860, and another, by Sir John Lubbock, to the Anthropological Institute of Great Britain in 1873. The former is entitled "On the Idol Human Head of the Jivaro Indians in Ecuador;" the latter, "Note on the Macas Indians." In Mr Bollaert's paper special attention is given to a specimen obtained by M. José Felix Barriero, of the Secretariat of the Provincial Government, Quito, and sent by him to Don R. de Silva Ferro, Chilian Consul, London. This specimen was placed in the Ecuador Court of the International Exhibition, 1862. Sir John Lubbock's paper is devoted chiefly to a general description of the Macas Indians.

The red specimen of shrunk head now exhibited was sent to my friend Dr H. Gunning of Palmeiros, Rio de Janeiro, a Fellow of this Society, by the Manager of the Amazon Steamboat Company, whom it had reached from the upper waters of that great river. It is styled by the sender a *Cabeza reduzida*—reduced head. Dr Alfred Pullar, owner of the black specimen, has kindly permitted me to show it to the Society. The former was obtained from the Pastaza tribe of Macas Indians, the latter from the Jivaro tribe. In a note accompanying the head, Dr Pullar says:—"Through the kindness of a friend in South America, I have lately received the curious war trophy of the Jivaros, who inhabit a remote district on the eastern side of Ecuador. It would appear that very few of these objects have been brought to Europe, their rarity being explained by the fact that their preparation is a religious custom of these Indians. Dr Duns, therefore, suggested to me that this trophy would be interesting to the Fellows of the Society in relation to a somewhat similar specimen which forms the subject of his paper. This curious preparation is a human head, which by some process, known only to the Indians, has been dried and reduced in bulk to its present condition. It consists of the entire scalp and face-skin, the features being perfectly preserved, the eyes closed, and eyebrows remaining intact. There is an aperture at the base of the head, through which the bones and other structures have been removed, leaving only the dried and shrunken integuments. The hair is of a deep black colour, very thick, soft and glossy, and about 20 inches in length. The measurements of the head are as follows:—From roots of hair or forehead to nose, 1¼ inches; nose to chin, 2 inches; from ear to ear, across nose, 4½ inches; length of ear, 1⅝ inches. The lips are sewn together, and a number of strings hang from them, the use of which is not apparent."

There is considerable difference between the red specimen and that thus described by Dr Pullar, both in measurement and other respects. I have brought them together for comparison, with the view of indicating features of which we have no record in the papers referred to above. While, of course, the accuracy of craniometric characterisations is not attainable and not sought here, yet the leading lines of skull measurement may be followed with regard to the integument, which still retains much of the shape it had before the bones were removed. In view of this explanation, I give the following, it being understood that the terms used are intended simply to indicate roughly the directions of the measurements :—

	Pastaza.	Jivaro.
Alveolo-condylian plane (Broca), . .	$3\frac{3}{8}$ inches	$4\frac{1}{8}$ inches
Vertical, or basilo-bregmatic diameter (Broca), .	$3\frac{1}{8}$,,	$3\frac{2}{8}$,,
Glabello-lambdoidean plane, or antero-posterior diameter, ...	3 ,,	4 ,,
Parietal diameter, ...	$2\frac{3}{8}$,,	$2\frac{4}{8}$,,
Nares, ...	$\frac{5}{8}$,,	$\frac{6}{8}$,,
Hair, ...	21 ,,	20 ,,

In the Pastaza specimen the septum of the nose and the eyebrows are awanting—the former evidently by injury after the head was prepared. In the Jivaro specimen the integument, at a point answering to about the middle of the coronal suture, has been squeezed together as if between the forefinger and thumb, thereby increasing the height of the forehead. In the other the compression is in the skin which covered the spot where the coronal and temporal sutures meet, thereby giving an aspect of greater breadth to the brow. The ears of the Pastaza specimen are smaller than those of the Jivaro, and in the ear lobes of both are holes, which show that large ear ornaments had been worn. The hair of both is thick, strong, tending to lanky, and of a glossy, deep black colour. Both have an exaggerated prognathous appearance. Does the difference of colour point to a tribal difference?

Artificial deformation of the head has long been noticed by ethnologists. It is widespread. Anciently it was not unknown in Europe, and it can now be traced in Polynesia, Asia, Africa, and both Americas. It was the expression of conventional ideas of beauty, as among the ancient Peruvians, and also among the still extant tribe of Flat-heads in North America; while, as regards the cutis, it is also met with among the Maories of New Zealand. It is practised on the dead as a mark of devotion to the memory of relatives among the Andaman Islanders; and, as a proof of success in war among the Dyaks of Borneo, the Jivaros and Pastaza's of Ecuador, and the Mundurucús of the Upper Amazon—facts which raise questions of much importance to ethnology, as bearing on the geographical distribution of tribes, and the import of the existence of identical customs and superstitions among nations far remote from each other. It is also worthy of notice that the Mundurucús should differ widely from their comparatively near neighbours, the Macas tribes, in the preparation of the head, and yet agree with the Bornean families. The Mundurucú does not attempt to remove the bones, or to reduce the natural size of the head. His effort is to preserve the natural likeness and the usual ornamentation. This is shown in the photograph on the table.

As regards the Ecuador specimens, it is impossible as yet to account for the fact, that we have so little information as to their meaning and the modes of preparing them, while so much has been recorded touching the several habits and industrial art of the tribes among whom they occur. In the reference to the Macas head, by Sir John Lubbock, the following passage occurs:—"The process of preparation, according to the account given me by Mr Buckley, is very simple. The head is removed, and, after being boiled for some time with an infusion of herbs, the bones, &c., are removed through the neck. Heated stones are then put into the hollow, and as they cool are continually replaced by others; the heat thus applied dries and contracts the skin. It will be seen that Mr Buckley's account confirms that of M. Barriero. A string is then run through the head, which is suspended in the hut, and solemnly abused by the owner, who is answered by the priest speaking for the head, after which the mouth is sewn up to prevent any chance of reply." But Sir John leaves out an important sentence in M. Barriero's account, who says:—"This is how I understood the matter; however, I may not have well understood the process." This theory of desiccation by the application of heated stones is not satisfactory. It is more likely the explanation will be found in connection with "the boiling for some time with a ninfusion of herbs." Rapid shrinkage might, perhaps, be thus obtained equal all over, and without folding, especially if accompanied by quick drying. And I may put a question in this connection which, however, I feel wholly unqualified to answer. Does what we know of the drying powers of the acid in certain vegetable oils, and of the action of tannic acid on skin, warrant a guess that there may be properties in the herbs used in these preparations which, being set free by boiling, might produce the result? The finger and thumb marks already referred to might be accounted for by the hypothesis that the heads were held over heated stones after being taken from the infusion of herbs. There is room enough to speculate. I think, however, the speculation is warranted by (may it not be said) the attainments of some of the Upper Amazon tribes in organic chemistry! How otherwise can we account for their skill to separate, by a complex process, the starch in the tubers of the manioc plant (*Janipha manihet*) from the strong narcotic poison they contain, and to give to the people the former as a nutritious bread—*farinha* or cassava—the "Bread of Brazil?" Or, even, how otherwise could they prepare from other plants the arrow poison, or *woorare?* It would not be a great surprise to learn that, having gouged the bones from scalp and face-skin, their skill as herbalists, aided by a little careful manipulation, enabled them to realise these shrunk heads. As regards their meaning, it seems to me we have information enough on this point to warrant the inference that they were used by the native priests as oracles, and that the strings, with their blood-red marks, were connected with this use.

Monday, 1st March 1886.

PROFESSOR DOUGLAS MACLAGAN, Vice-President, in the Chair.

The following Communications were read:—

1. On the Magnitude of the Mutual Attraction between two pieces of matter at distances of less than ten micromillimetres. By Sir William Thomson, Hon. V.P.

2. On a Theorem in the Science of Situation. By Professor Tait.

3. On Radiation from Snow. By John Aitken, Esq.

4. Comparison of the Volumes of Saline Solutions with the Sums of the Volumes of the Constituents. By J. Holms Pollok, Chemical Laboratory, Glasgow University. Communicated by Sir William Thomson. (Plate XXII.)

My attention was first drawn to the change of volume which takes place when a salt is dissolved in different quantities of water, by an experiment shown by Sir William Thomson in his class at the Glasgow University. He took a wide glass tube, about 3 feet long, sealed at one end, and having a narrow tube attached to the other end. The lower half of this tube he filled with a saturated solution of common salt, and on the top of that he placed a layer of water, which just reached a mark about the middle of the small tube. He then corked the open end, and mixed up the water and brine by inverting the tube two or three times, when it was seen that the solution had contracted.

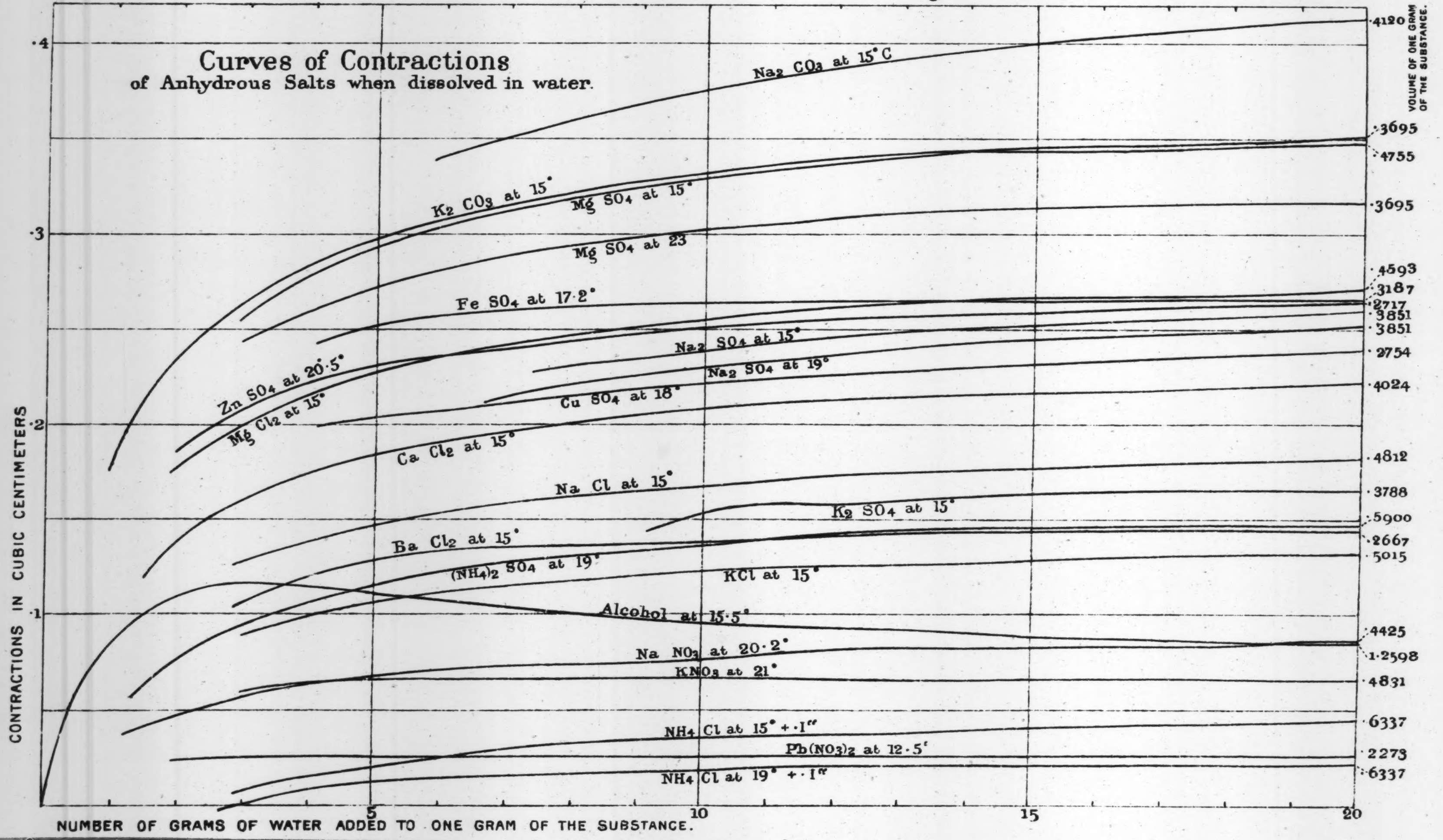
After seeing this experiment, I determined to find the amount of contraction that takes place; but, instead of taking a saturated solution, I took 1 gramme of the anhydrous salt, and ascertained the amount of contraction that takes place on dissolving it in any quantity of water from 1 gramme to 20.

These results I calculated from the tables of specific gravities of solutions of salts, given in Storer's *Dictionary of Solubilities*. Those for the curves at 15° were obtained by Gerlach, with the exception of those for alcohol, which were taken by Fownes; the specific gravities for the others were obtained by Schiff, with the exception of those for lead nitrate, which were by Hassinfratz. The contractions were found by means of the formula

$$C=\frac{A}{a}+\frac{B}{b}-\frac{A+B}{r},$$

where C is the contraction in cubic centimetres, A the number of grammes of the substance taken, B the number of grammes of water added, and a, b, and r the specific gravities of the substance, the water, and the resultant solution, all referred to 4° C.

Thus, I find by calculation in the case of magnesium sulphate at 15° C., that taking 1 gramme of $MgSO_4$, and adding to it 2·961



Curves of Contractions
of Anhydrous Salts when dissolved in water.

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grammes of water which gives a saturated solution, the contraction which takes place amounts to ·255 c.c., on adding sufficient water to make this 3 grammes the contraction increased to ·256 c.c., and on adding another gramme it increased to ·278 c.c., and so on until 20 grammes of water were added, when the contraction amounted to ·350 c.c. These results I have represented by curves, the ordinates giving the contractions in cubic centims., and the abscissas the number of grammes of water added to produce those contractions; the curves start at the point of saturation of the solutions, and the height of this point above the line of abscissas represents the amount of contraction that takes place when 1 gramme of the substance is dissolved in as small a quantity of water as possible. It will be noticed that there is contraction in every case with the exception of ammonium chloride, which gives an expansion on dissolving, but as water is added the amount of this expansion diminishes—that is, a solution when diluted contracts. In order to bring the curves for ammonium chloride into the diagram, I have added ·1 c.c. to each of them, which must therefore be subtracted in order to obtain the true change of volume.

From the appearance of the curves and by trial, I find that they are portions of equilateral hyperbolas having asymptotes parallel to x and y, and may be represented by the general equation

$$y = b - \frac{C}{a+x},$$

where y is the contraction in cubic centims., x the number of grammes of water added, a the distance from y of the asymptote parallel to y, and b the distance from x of the asymptote parallel to x, and C the constant $(a+x)(b-y)$; thus,

for $MgSO_4$ $a = 1{\cdot}429$ $b = {\cdot}374$ and $C = {\cdot}500$,
for $MgCl_2$ $a = 1{\cdot}645$ $b = {\cdot}290$ and $C = {\cdot}408$,
for $CaCl_2$ $a = 1{\cdot}857$ $b = {\cdot}240$ and $C = {\cdot}391$,
for K_2CO_3 $a = 1{\cdot}644$ $b = {\cdot}374$ and $C = {\cdot}501$.

Although the curves of contractions for all salts appear to be portions of equilateral hyperbolas, other substances give very different curves. Thus alcohol contracts until you have added 3 parts of water, and then the amount of contraction begins to diminish;

therefore, if we take a mixture of 3 parts of water and 1 part of alcohol and add water to it, we get an expansion. In cases where I have drawn the curves for two different temperatures, the effect of increase of temperature is to diminish the amount of contraction.

On the right hand side of the diagram the volume of 1 gramme of the salt in cubic centims is placed at the extremity of each of the curves, and if it be desired to obtain the increase of volume of a given weight of water on adding to it 1 gramme of a salt, it is only necessary to subtract the contraction from this number, and you get the increase of volume. From this it will be observed that Dalton's theorem—that the volume of water was not increased by dissolving in it an anhydrous salt capable of taking up water of crystallisation —does not hold good in any of the cases taken, except that of sodium carbonate; and there it only holds good provided the salt be dissolved in not less than twenty times its own weight of water.

P.S.—Since writing the above I find that in 1846 a paper was published by John Joseph Griffin, on the "Constitution of Aqueous Solutions of Acids and Alkalies," in which he points out that the volume occupied by a substance in solution varies with the quantity of water in which it is dissolved, thus controverting Dalton's theorem. In this paper he gives contractions of the common acids and alkalies, and also of K_2CO_3, Na_2CO_3, NH_4Cl, and $MgSO_4 + 7H_2O$ (*Memoirs of the Chemical Society*, vol. iii. p. 155).

5. On the Increase of Electrolytic Polarisation with Time.

By Mr W. Peddie.

The law of variation of the electromotive force of polarisation with current-density has been very thoroughly investigated. It is found that it may be represented by an equation of the form

$$F = a - b\varepsilon^{-cx},$$

where x is the current-density, and a, b, c are constants. So for very feeble direct currents the reverse polarisation current is proportional to the current-density; but for strong currents it cannot be expressed by an algebraical formula as a function of the current-density in a finite series of terms. The above formula gives the *maximum* value of the polarisation current. So far as I know, no observations have been made regarding its variation with time from its commencement until the maximum is reached. This question is of interest chiefly from a theoretical point of view. The object of this note is to describe the result of some experiments made on this subject. Platinum electrodes, heated to redness before each experiment, were used. The electrolyte was a 5 per cent. aqueous solution of sulphuric acid. A Helmholtz galvanometer was placed in circuit with the electrolytic cell and the battery. The reading of the galvanometer was taken every minute from the time of starting the current until it apparently reached the stationary value. The results of two of these experiments are shown graphically by the subjoined curves. The ordinates are proportional to the current-strength, and the abscissæ represent time. These curves closely resemble the logarithmic curve in form. To test how close the correspondence is, we may assume the equation

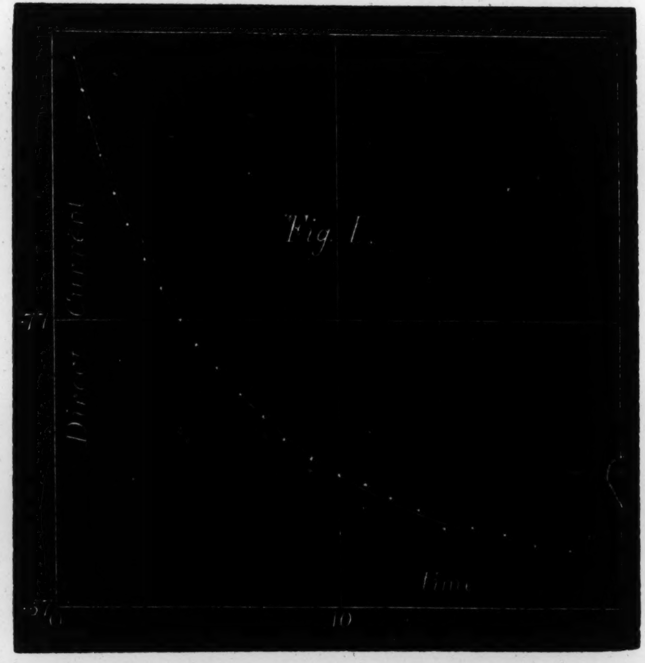


Fig. 1.

$$j = a - b\varepsilon^{-ct}$$

to be true, when j is the polarisation current, t is the time, and a, b, c are constants. In fig. 1, I have taken the point (·85, 1) as the origin, and reckoned j downwards to the curve from a line drawn parallel to the axis of time through this point. The value of the constants may be got from three points $(j_1, t_1) \ldots (j_3, t_3)$, where $t_1 \ldots t_3$ are in arithmetical progression, by means of the following equations:—

$$a = \frac{j_1 j_3 - j_2^2}{j_1 + j_3 - 2 j_2}$$

$$\log b = \frac{t_2 \log (a - j_1) - t_1 \log (a - j_2)}{t_2 - t_1}$$

$$c = \frac{\log (a - j_1) - \log (a - j_2)}{(t_2 - t_1) \log \epsilon}.$$

For the curve in fig. 1, I obtain the following equation by means of points between $t = 10$ and $t = 19$—

$$j = \cdot 162 - \cdot 165 \epsilon^{-\cdot 19 t}.$$

The position of this curve is shown in the figure by means of the dotted line.

Fig. 2 shows the results of another experiment, in which the current-density was different. Here the point (·37, 0) is taken as origin. The equation to this curve calculated from the points corresponding to $t = 5$, $t = 7$, ... on the assumption that it is a logarithmic curve, is

$$j = \cdot 116 - \cdot 101 \epsilon^{-\cdot 218 t}.$$

It will be seen that this curve coincides with that obtained by experiment except near the origin. I find that the observed points lie very accurately on a curve whose equation is of the form

$$j = a - b \epsilon^{-ct + d\epsilon^{-et}}.$$

This curve practically coincides with the former for values of t greater than 5, so that we may assume a, b, and c to have the same values in both. To determine d and e we have the equations

$$\log \frac{b}{a - j} = ct$$

$$\log \frac{b}{a - j'} = ct - d\epsilon^{-et},$$

where j and j' correspond respectively to points on the logarithmic curve, and on the curve representing the results of observation for the same value of the time. Hence

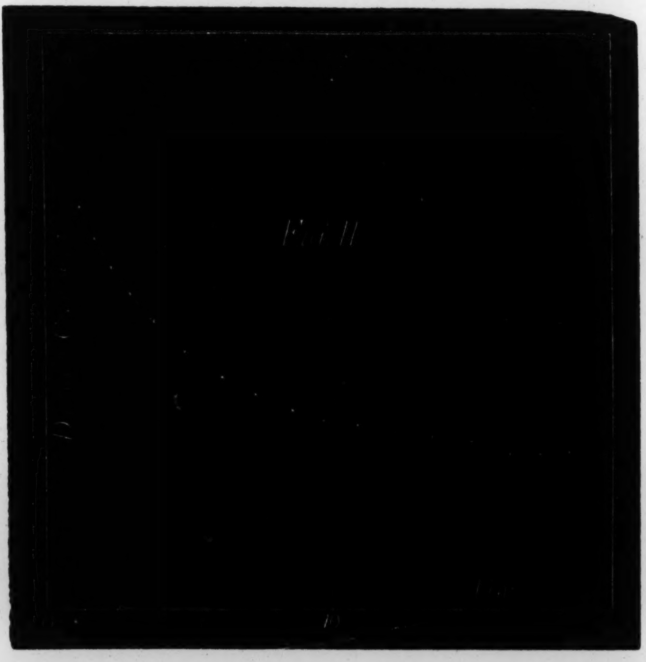
$$e=\frac{1}{t'-t}\log.\log\left(\frac{a-j'}{a-j}\right)_t\left(\frac{a-j}{a-j'}\right)_{t'},$$

$$d=\frac{\log\left(\frac{a-j}{a-j}\right)_t}{\varepsilon^{-ct}}.$$

For the curve of fig. 2 we get the equation

$$j=\cdot 116-\cdot 101\varepsilon^{-\cdot 218t+\cdot 131\varepsilon^{-\cdot 53t}}.$$

This curve is practically indistinguishable from that of fig. 2. The sign of d will determine whether the logarithmic curve lies above the actual curve or not. But all the curves I have obtained lie above the logarithmic curve with the exception of that in fig. 1; so that, especially since the curves in that figure are apparently again approaching each other, I believe that this apparent exception is really due to an error in drawing the curve through the observed points where t is large, which would affect the constants.



It would seem, then, that the polarisation current can be expressed as a function of the time by means of a formula of the form

$$j=a-b\varepsilon^{-ct+d\varepsilon^{-et}},$$

where d is always positive.

My thanks are due to Messrs Moffat and Callender, students in the University Physical Laboratory, who have conducted the experiments for me.

Added June 23.—Further experiments made with larger electrodes show that the above empirical formula closely represents the results of observation; but they also show that the curves are symmetrical about an axis, and are probably hyperbolas. I am at present engaged in investigating the point.

6. On Thermometer Screens. By John Aitken, Esq.

(Plate XXIII.)

PART III.

In my previous communications to the Society on this subject* some different forms of thermometer screens are described, and the results are given of a number of trials made with them during trying conditions of weather. The result of these tests showed that all the screens gave readings a little too high when there was much radiation, and that the thermometer in the fan apparatus also indicated too high a temperature, the readings of a fine-bulbed thermometer in a polished silver case being taken as our standard of temperature in these trials. The reasons are given for the thermometer in the draught screens indicating too high temperature, and the ineffectual attempts made at the time to check these errors are described.

Confining our attention for a moment to the draught screen, fig. 3, and the fan apparatus, figs. 5 and 6, of previous papers. The reasons for the thermometer in screen fig. 3 reading too high are—1st, the outside of the case being heated by the sun, part of this heat is conveyed inwards to the thermometer, being conducted through the concentric tubes, and radiated from tube to tube, the air passing over the surface of the tubes not being able completely to check this transference of heat; 2nd, the too high readings given by this screen are also in part due to the heat radiated and reflected from the ground, and entering the lower end of the tube. The introduction of small screens into the tube below the thermometer to check the entrance of radiant heat, while admitting a free passage of the air, was not found to improve the correctness of the readings given by this screen. The entering air seemed to be heated as much by the screens as the bulb was by direct radiation.

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* *Proc. Roy. Soc. Edin.*, No. 117, 1883-84.

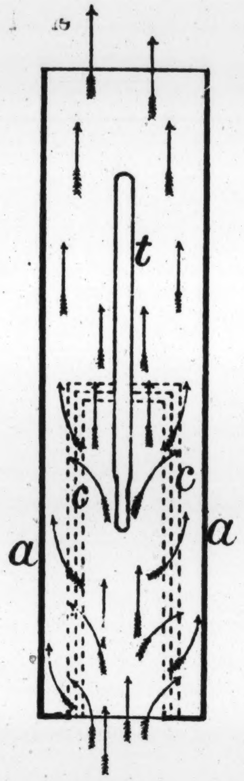

Fig 1

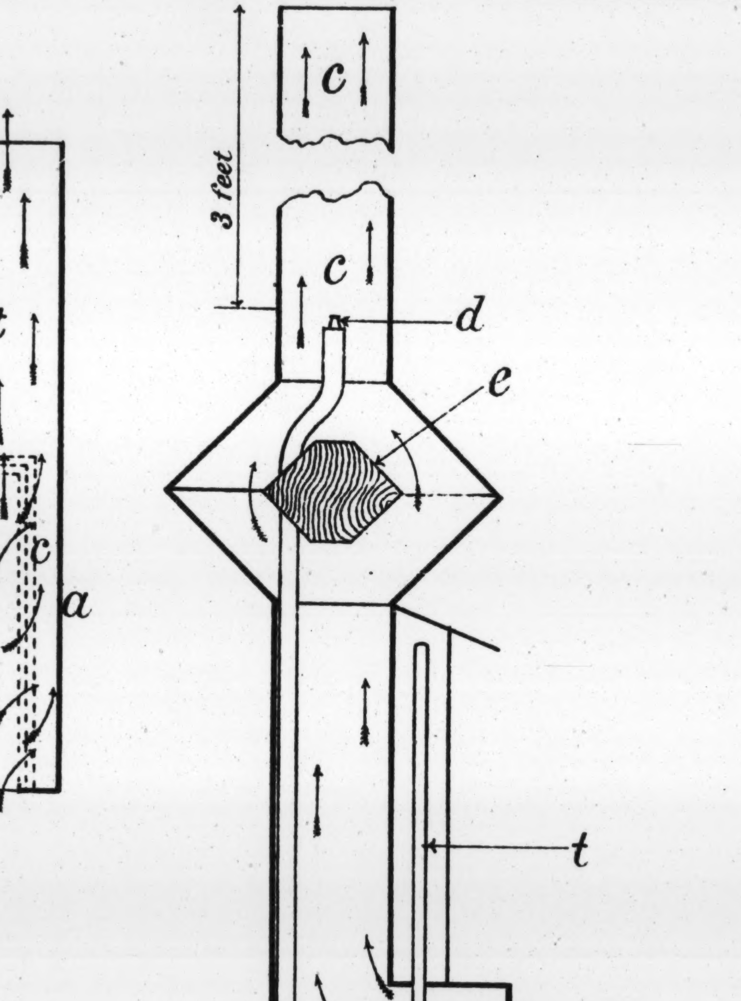

Fig. 2.

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A third reason of the too high readings is that some of the air heated on the outside of the case is driven by air currents past the annular openings and enters the central tube. The too high readings given by the fan apparatus would be principally due to the heat conducted through the walls of the air passage and radiated to the thermometer, and very little of it to heat radiated by outside objects entering by the air passage.

In the attempts to devise arrangements to correct these errors, I have been guided very much by two conclusions arrived at in a previous part of this investigation. The one conclusion was, that if we wish a body to acquire the temperature of the air it must be infinitely small, and in practice it should be as small as possible; the smaller it is the nearer will its temperature be to that of the air. The other conclusion was, that all radiant heat must be prevented from falling on the bulb of the thermometer, because the radiant heat is absorbed, not only at the surface, but also internally, by the glass walls of the bulb, and the passing air carries away heat only from the outside of the bulb, which explains the reason why, in practice, it was not found possible to check the heating effect of a very small amount of radiation by any velocity of air current that could be made to pass over the bulb.

This investigation thus naturally divides itself into two parts. The first, how best to check the heat absorbed by the outside of the case, and prevent it getting to the thermometer; the second, how best to check the entrance of radiant heat at the opening by which the air enters.

Taking these two points in their order, we shall confine for the present our attention to the question how best to prevent the thermometer being heated by the hot case by which it is surrounded. In the screen shown in fig. 3 of the previous paper, the transference of heat was checked by the non-conducting concentric tubes and air passages. The cooling effect of the air in this arrangement cannot, however, be perfect, on account of the length of the surface over which it flows in an even stream; only the lower edges, where the air first touches these tubes, will be cooled to near the temperature of the air, the upper parts being warmer and warmer the further they are away from the entrance.

Guided by the principle already referred to, it appeared that the best method of causing the walls of the tube to acquire the temperature of the air, and at the same time prevent heat passing from the outer case to the thermometer, would be to surround the bulb with a tube of some open or porous material, such as cloth, through which the air could be made to pass, and to cause the air to enter one end of this tube, and to pass radially through its walls in the manner shown in Plate XXIII. fig. 1, where *a* is the outside case heated by radiation, *t* the thermometer, and *c* a tube of some porous material surrounding the thermometer and placed inside the draught tube as shown. The arrows in the figure show the direction of the air currents in the draught tube under these conditions, from which it will be seen that the air currents are made to flow in a direction the opposite of that in which the heat tends to move; and it was expected that the outward-moving air currents would check the inward movement of the heat, and prevent it arriving at the thermometer; also that the passing air would cause the inside surface of the tube rapidly to acquire the temperature of the air, and to follow its changes quickly.

The trials with this arrangement were quite successful. The advance of heat was so perfectly held in check by it that the thermometer kept at the temperature of the air, even when the surrounding tube was highly heated. As these experiments may have some interest, I shall give somewhat in detail the results of some experiments made with some different materials used for this purpose. As it was winter, and no experiments of this kind were possible in the open air, a draught screen was prepared for making the trials in the laboratory. This apparatus is shown in fig. 2: *a* is the case surrounding the thermometer *t*; *c* is the draught tube, heated by a gas jet *d*, for promoting a circulation of air; *e* is a screen placed below the gas flame to prevent heat being radiated downwards to the thermometer case. The air enters the lower end of the case, and is drawn upwards by means of the column of heated air in the draught tube *c*. As it was necessary to put this method of protecting the thermometer to as severe a trial as possible, the outer case *a*, surrounding the thermometer, was jacketed by the case *f*, which could be kept hot with steam. The thermometer could be thus exposed to radiation from a surface all round it having a temperature of 100° C.

In experimenting on this method of protecting the thermometer, different materials were used for making the porous tube; the results will be here given of the trials with muslin, wire-cloth, and cotton wool. The material under trial was rolled up in the shape of a tube and placed inside the case of the draught tube, as shown at *g*, and the protecting power of one, and of a number of thicknesses of the different materials was measured.

As the question to be determined was, how much the thermometer surrounded by the hot case was heated above the temperature of the air when protected in this manner by the different materials, it was necessary that the temperature of the air should be very accurately determined. In conducting the investigation, it soon became evident that a thermometer hung up in the room could not be relied upon for this purpose, as it was too much affected by radiation, from the fire, from gas flames, and from the body of the observer; for the amount of difference required to be measured was not degrees, but fractions of a degree. Another draught screen was therefore prepared, and a thermometer placed in it. The thermometer in this screen was well protected by concentric tubes; a gas flame placed close to the case had no effect on it, and all radiation from below was easily checked. The draught was kept up in this screen, as in the other, by means of a jet of gas in the upper part of the tube. The temperature of the air as given by this screen could be relied upon to a small fraction of a degree. In the trials, the two draught screens were placed as near each other as convenient, generally about one-third of a metre apart.

The thermometers used in these trials were graduated to Centigrade degrees. The scale of both of them was very wide, and each degree was divided into tenths—the scale of the one showing the temperature of the air was 4·2 mm. to the degree, and the other 5·5 mm. to the degree. The thermometers were carefully compared with each other in water, and corrections made for difference.

In experimenting with this arrangement of apparatus, the gas was lighted in both draught tubes to produce a circulation of the air. In the screen under trial a No. 1 jet was used turned down to one quarter of full flame, and kept as constant as possible during the trials. The first trials were made with the thermometer case as it came from the work, but without polishing the inside surface that radiated heat to the thermometer. A number of trials were made with it in this condition, of which it is unnecessary to give the detailed results. It may, however, be mentioned, that with the usual draught and with no protection between the thermometer and the walls of the case—that is, with the bulb exposed all round to radiation from bright tin at 100°—the thermometer rose to 18°, the temperature of the air being 12°; that is, it was heated 6° above the temperature of the air. When one thickness of wire-cloth and two thicknesses of muslin were placed between the bulb and the case, the radiation was almost entirely checked, and the thermometer rose only about 0°·2 above the temperature of the air.

As it could not be expected that the inside of the case would keep bright after being long in use, it was thought advisable to make the tests under the most disadvantageous conditions possible. The inside of the case was therefore painted black. When this was done, a great increase in the heating effect on the thermometer was produced, as was to be expected.

In the following tables are given the results of some experiments made with the inside of the case blackened, and with fine cloth as the protecting material. The protection effected by different numbers of thicknesses was tried, and the results are given. The cloth used in these trials was book-muslin, about 56 meshes to the inch.

Muslin.

1. Air.	2. Case.	3. Difference.	4. Protection.
13°·4	31°·6	18°·2	None
13°·4	20°·9	7°·5	1 Thickness
13°·4	17°·4	4°·0	2 ,,
13°·25	13°·6	0°·35	4 ,,
13°·3	13°·3	0°·0	12 ,,

In the above table, in column 1 is given the temperature of the air of the room; in column 2 the reading of the thermometer surrounded by the case heated to 100°; in column 3 the difference between the thermometers, and shows the amount of heating produced by the warm case; and in column 4 the protection placed between the thermometer and the hot case.

It will be observed that when there was no protection between the thermometer and the case, the thermometer got heated 18°·2 above the temperature of the air, or three times more than it was before the radiating surface was painted black. It will be noticed that a single thickness of muslin placed between the thermometer and the case greatly checked the radiation; that as the number of thicknesses was increased, the heating effect rapidly diminished; and when twelve folds or thicknesses were between the two, not the slightest effect could be detected. This test has been repeated a number of times with the same result. To check the results, readings were taken with the steam off and the case cold. Steam was then turned on, the case heated, and readings taken. Steam then cut off, and cold water circulated through the jacket, but not the slightest difference could be detected under the two conditions, showing that the thermometer was thoroughly protected by twelve thicknesses of muslin. The protection was of course perfect only under the condition of the experiment. If the draught was reduced by shutting off the gas, and the air heated only by the small amount it got in its passage through the case, then the thermometer rose a very little above the temperature of the air.

No comparative trials were made with black and white muslin, as it was not possible to get two muslins similar in every respect except colour; but as we are here dealing with dark heat, it does not seem likely that this would have any effect.

The next two tables give the results of tests made with two different kinds of wire-cloth as the protecting material. In the first table are given the results obtained with wire-cloth made with No. 35 iron wire and 36 meshes to the inch; in the second table the results obtained with No. 38 brass wire and 70 meshes to the inch.

The contents of the different columns in these two tables are arranged as in the last one. It will be observed that the results with wire-cloth are not so good as with the muslin; this will be due partly to the conducting power of the wire, and also probably in part to heat reflected by the polished surface of the wires. These tables show very little difference in the protecting powers of the two wire-cloths. The tables must not, however, be compared too closely, as the conditions do not admit of very exact results. It may be noticed, for instance, that while the two tables agree fairly well at the two ends, they do not agree at intermediate points. This disagreement at the intermediate points will probably be the result of the more or less perfect overlapping of the meshes, and of the greater or less degree of contact between the folds of the cloth. Neither of these have any existence when only one thickness is used; they are averaged when there are many thicknesses, and

Iron Wire-Cloth.

No. 35 wire and 36 meshes to the inch.

1. Air.	2. Case.	3. Difference.	4. Protection.
15°·7	34°·4	18°·7	None.
15°·4	25°·7	10°·3	1 Thickness.
15°·3	22°·5	7°·2	2 ,,
13°·6	16°·3	2°·7	4 ,,
13°·4	14°·0	0°·6	8 ,,
15°·0	15°·05	0°·05	12 ,,
15°·2	15°·2	0°·00	16 ,,

Brass Wire-Cloth.

No. 38 wire and 70 meshes to the inch.

1. Air.	2. Case.	3. Difference.	4. Protection.
15°·6	34°·5	18°·9	None.
16°	26°·6	10°·6	1 Thickness.
16°·4	22°·8	6°·4	2 ,,
16°·3	18°·8	2°·5	4 ,,
16°·4	16°·8	0°·4	8 ,,
16°·1	16°·15	0°·05	12 ,,
16°·2	16°·2	0°·00	16 ,,

it is only at the intermediate points that they have a disturbing influence.

Cotton Wool, Medium Quality.

1. Air.	2. Case.	3. Difference.	4. Protection.
14°·0	32°·6	18°·6	None.
13°·8	13°·8	0°·0	1 Thickness.

The above table shows the result with cotton wool. The kind known as medium quality was used, the sheets of which are not so thick as those of a finer kind. For supporting the cotton wool in its place inside the case, a wire-cloth cage was used, its inside being lined with one thickness of the wool from which the *skin* had been previously removed. This extremely feeble-looking protection, as will be seen from the table, acted perfectly; the thermometer when protected by it was quite unaffected by the hot case. A number of readings were taken when the case was heated to 100°, and when cooled with water, but not the slightest difference could be detected. Without any gas to assist the draught, the temperature rose 0°·2 above the temperature of the air.

It will be noticed that a very thin covering of cotton wool or muslin, when employed in the manner here described, is quite sufficient to stop the advance of all heat from the warm walls of the case surrounding the thermometer; and further, it is interesting to notice that the amount of muslin and cotton wool that stops all heat is far from being able to stop all light. A good deal of light can come through a sheet of cotton wool, or twelve thicknesses of muslin. This peculiar action reminds us of the somewhat similar action of glass and other substances, which pass the rays of one end of the spectrum, while they stop those of the other.

When we consider the action of muslin used in this manner to check radiation, we see it is one that has been long well known. Gardeners are well acquainted with the fact that any cloth, however thin, spread over plants, is a great protection to them on frosty nights. The action of the cloth on these occasions is exactly the same as in the thermometer case; the thin cloth checks radiation, and prevents the heat from passing into space. The cloth itself is but little cooled by radiation, as it is constantly receiving heat from the air passing through its meshes, the air on these occasions being always hotter than the exposed surfaces of bodies. The plant therefore makes its heat-exchanges with a surface but little cooled by radiation instead of with the cold of space, as tempered by our atmosphere. We see from this that the protection afforded by these cloths on frosty nights is efficient only on what are called radiation frosts; the cloth has no power to heat the air. Further, we see that the efficiency of the cloth will be a good deal affected by the amount of wind; the better the air circulation the warmer the cloth will be—that is, the less it will be cooled by radiation. It may also be observed that an open cloth may be under certain conditions a better protection than a close covering. From the experiments with muslin in the draught tube it was seen that one thickness of this material reduced the radiation effect to less than half. From experiments made at night with one radiation thermometer, protected by a piece of muslin stretched horizontally at a little distance above it, while another was freely exposed to the sky, I found that one thickness of the same muslin reduced the cooling effect of radiation to almost exactly one half, the less circulation of the air at the time accounting for the difference.

It is now long since Dr Joule first laid down certain imaginary conditions under which it might be possible to get the true temperature of the air. The method proposed by him was to place the thermometer in a long vertical tube, open at both ends, but with a cap by which the lower end could be closed. This tube was to be surrounded with a jacket by means of which it could be heated to any desired temperature. The temperature of this tube was to be carefully adjusted, so that the air inside of it tended neither to ascend nor to descend when the cap was taken off. When this condition of matters was arrived at, the air inside the tube would evidently have the same temperature as the air outside of it, and the thermometer would be at the true temperature of the air, as the radiating surfaces all round it would have the same temperature as the air. It is needless to say these conditions could not be carried into practice, for taking observations except where the temperature was nearly constant, and the air free from currents; in the open air, winds and the constant changes of temperature there taking place would make it quite impracticable.

By the method here described we do seem to have a practical way of surrounding the thermometer with the air to be tested, and of causing the surrounding tube to take up and follow the changes in the temperature of the air. By means of muslin, cotton wool, or wire-cloth, through which the air is made to circulate, it does seem possible to surround the thermometer with a tube at the temperature of the air, and to protect it from all radiation from surrounding objects. Under the conditions existing in the open air, where the case is never very highly heated, the error, when the thermometer is protected in this manner, need never amount to more than 0°·01, or less than a readable amount on all thermometers in general use.

But though we can by the above means surround the thermometer with air at the correct temperature, and with a tube at the temperature of the air, yet our difficulties are far from being at an end. We unfortunately cannot get the air into and out of the tube without openings, and these openings either allow radiant heat to fall on the thermometer, or if we place screens to check the radiation, then these screens heat the air in its passage over them; the air and the inside walls of the tube get thereby heated, and the thermometer gives too high a reading.

The investigation has, therefore, been continued in this direction, and some preliminary experiments made on different methods of checking the entrance of radiant heat; it is unnecessary, however, to give here more than a brief outline of the results. For making experiments on this radiation effect, the lower end of the draught tube was placed horizontally with its opening in front of a large gas flame, which was allowed to radiate freely to the thermometer, the upper part of the draught tube being, as before, vertical, to produce the required draught. With the draught produced by a small jet of gas, the error due to heating by radiation from the flame was 0°·45. Screens were then placed inside the case, like louvre boards, to prevent the radiation falling on the bulb, but their presence was found to increase the error. The effect of a strong draught of air produced by a fan was then tried, the screens being removed and the flame allowed to radiate direct to the bulb, but the effect of the powerful blast was only to reduce the error from 0°·45 to 0°·35; that is, the strong draught only gave a reduction of $\frac{1}{10}$th of a degree more than the slow draught of the gas jet. This inability of a strong draught of air to check the effects of radiation has been noticed in a previous part of this investigation. With the screens replaced in the tube in front of the thermometer and fan draught the error was 0°·3.

As the introduction of screens between the source of the radiation and the thermometer bulb produced very little improvement even with a strong draught, probably because the entering air got as much heat on the warmed screens as the bulb did by direct radiation, an arrangement was devised in which all air that had touched the screens, or sides of the case, was drawn through side passages, and only the central core of the entering air allowed to pass to the thermometer. When this arrangement of apparatus was tried, the error with gas draught was reduced to 0°·15, and with fan draught to only about 0°·05.

PRIVATE BUSINESS.

Mr Hugh Miller, Mr John Richard Brittle, the Right Hon. the Lord Provost, Professor Armstrong, Professor Wallace, Dr Arthur Anderson, C.B., Mr Alexander Gibson, Advocate, Colonel R. Murdoch Smith, R.E., and the Right Hon. the Earl of Haddington were balloted for, and declared duly elected Fellows of the Society.

Monday, 15*th March* 1886.

SHERIFF FORBES IRVINE, Vice-President, in the Chair.

The following Communications were read:—

1. The Volumetric Estimation of Inorganic Nitrites. By G. Armstrong Atkinson, M.B., C.M., Assistant to the Professor of Materia Medica in the University of Edinburgh.

The ever-increasing importance of nitrites in medicine, owing to their power as therapeutical agents, and to the scientific work which has been and is being done to elucidate their pharmacology, renders a knowledge as to the precise amount of nitrite a commercial specimen contains of high importance. For while to most patients a dose of five or six grains of pure, or nearly pure, nitrite of sodium is as large a quantity as they can conveniently bear, we read[*] of twenty and thirty grains of so-called nitrite of sodium having been administered without dangerous or very unpleasant results. In such cases we cannot but conclude the drug was impure, and MacEwan[†] has shown that samples of nitrite of sodium were met with in commerce containing but a decimal percentage of the salt. Many papers[‡] have recently appeared on the estimation of nitrite of ethyl in the spirit of nitrous ether, but in this contribution I desire only to consider those methods suitable for the estimation of nitrous acid in its inorganic compounds. While many methods are employed for this purpose, that chiefly followed in pharmaceutical chemistry is one or other of the modifications of the permanganate process, a process first applied to the volumetric estimation of inorganic nitrites by Feldhaus.[§] He presumably obtained the idea from Forchhammer of Copenhagen, who in 1850 described a process for the estimation of organic matter in potable waters by permanganate of potassium. The process, as applied to nitrites, depends upon the power of permanganate of potassium, especially in the presence of free acid (preferably sulphuric), to oxidise nitrous to nitric acid, as may thus be expressed in the case of nitrite of sodium:—

$$4H_2SO_4 + 2KMnO_4 + 5NaNO_2 = 2MnSO_4 + 2KHSO_4 \\ + 5NaNO_3 + 3H_2O .$$

Taking the same example, it is obvious that to estimate the percentage of actual nitrite in commercial nitrite of sodium, all we require is to ascertain the amount of permanganate (representing a certain quantity of oxygen) which a given quantity of the commercial nitrite can decolorise, and compare it with the quantity which would be decolorised by an absolutely pure nitrite. This latter quantity is readily enough ascertained from the atomic weights of the elements. The simplest method of estimating the amount of permanganate, and therefore of oxygen required, is to follow such a process as Feldhaus describes in a later paper[*] than that I have referred to. He recommends the nitrite to be dissolved in very slightly acidulated water, and permanganate of potassium to be added until the oxidation of the nitrous acid is nearly completed, when the solution is to be made strongly acid, and permanganate run in until a permanent light red coloration is obtained. Now, as Fresenius[†] points out, and as very little experience teaches us, nitrous acid, owing to its great tendency to decompose in watery solution with the formation of nitric acid and nitric oxide, requires in such an estimation as that we are considering to be present in the amount of not more than 1 in 5000 of water, and even with this, and a much greater degree of dilution, decomposition slowly proceeds. Furthermore, we must remember anhydrous nitrous acid (N_2O_3)—we cannot speak of the hydrated acid (HNO_2), as it is not known in the pure state—has a boiling point of about $-10°$ C.,[‡] so that the risk is incurred of the estimation being vitiated not only by decomposition, but by decomposition and volatilisation combined. After numerous experiments with this method of Feldhaus, I have not obtained satisfactory results. The slowness of oxidation which occurs when the feebly acidulated solution contains but little permanganate, gives time for those evils—volatilisation and decomposition—of which I have just spoken, to occur. Moreover, since there is some difficulty in knowing when the reaction is almost, and even more when it is quite, completed, I have been led to look for a modification of the permanganate process—a modification which will allow very rapid oxidation of the nitrous to nitric acid. To add some excess of permanganate, and estimate that excess by a reducing agent, has been frequently described, but I obtain the most satisfactory results by using a large excess of permanganate, and titrating back with some suitable reducing agent. Such an agent may be found in oxalic acid, as suggested by Kinnicult and Nef,[§] or in ammonio-ferrous sulphate, as employed by Feldhausen and Kubel.[||]

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[*] See especially the *Practitioner*, vol. xxx. p. 109.

[†] *Pharmaceutical Journal*, 3rd series, vol. xiii. p. 121.

[‡] Consult papers by Dott, MacEwan, Eykman, and Allen, in *Pharmaceutical Journal*, 1882–85.

[§] *Archiv der Pharmacie*, April 1860.

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[*] *Zeitschrift für analytische Chemie*, vol. i. p. 426.

[†] *Anleitung zur quantitativen chemischen Analyse*, 6th edit., vol. i. p. 390.

[‡] Dittmar, *Chemical Analysis*, p. 224.

[§] *American Chemical Journal*, vol. v. p. 388.

[||] Classen, *Quantitative Analyse*, p. 222.

Or we may, as Tidy * recommends in a process for the estimation of organic matter in potable waters, add iodide of potassium, and estimate the liberated iodine with hyposulphite (thiosulphate) of sodium, adding starch solution to indicate the exact point at which to stop. Muter † uses a very similar method for the estimation of the nitrite in spirit of nitrous ether. I have carefully compared all these methods, and unhesitatingly give preference to that in which ammonio-ferrous sulphate is the reagent used, as it, while quite as accurate, is more easily employed than either of the other two Assuming a temperature of 60° F. to be taken as granted, the process, as I use it, requires the following solutions, the most convenient strengths of which have been fixed after repeated trials with solutions containing varying amounts :—

1. *Solution of Permanganate of Potassium*—one gram in the litre.—This solution, although recrystallised permanganate is usually almost pure, must have its exact strength calculated by titration in one or other of the ordinary ways,—preferably, I think, against metallic iron or a ferrous salt, although oxalic acid has a powerful advocate in Berthelot.‡

2. *Solution of Ammonio-ferrous Sulphate.*—It is most convenient that this solution be of such a strength that 1 c.c. will be peroxidised by, and will therefore decolorise 1 c.c. of the permanganate solution. To do this we require roughly 12·5 grammes of the iron salt in the litre (12·4339 precisely). The mere watery solution soon decomposes, but I find the addition of 1 c.c. of pure sulphuric acid for each gramme of the salt employed (that is, I add 12·5 c.c. of acid for each litre of the solution) keeps the solution well, and also furnishes acid to assist the acid already added in decomposing the permanganate, and in converting the ferrous into the ferric salt. Each day the solution is used it must, however, be titrated against the permanganate, a process which occupies only a few minutes.

3. *Diluted Sulphuric Acid.*—That of the British Pharmacopœia does quite well,—it is almost 1 in 12, but I am in the habit of using a 10 per cent. solution of pure acid. It is very necessary to be certain that the solution will not itself decolorise some permanganate, as impure sulphuric acid so generally does.

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* *Journal of the Chemical Society*, vol. xxxv. (new series), p. 46–106.

† *Pharmaceutical Journal*, vol. x. (3rd series), p. 94.

‡ Dingler's *Polytechnisches Journal*, ccxii. p. 354.

4. *A one per mille Solution of the Nitrite to be investigated.*—In the case of such nitrites as nitrite of silver, where the metal has a high atomic weight (Ag = 108), it is advisable to use 2 per mille. But as usually nitrite of sodium or potassium is the nitrite in question, 1 in 1000 is the most suitable strength. Now, as we are dealing with the action of reducing agents on permanganate of potassium, a question first started, so far as I know, by Jones,* comes under consideration. He states that the action of oxalic acid, of ferrous sulphate, and of some other substances with which he has experimented on permanganate of potassium, in strong or in dilute solution, and with or without the presence of sulphuric acid, is accompanied by the evolution of oxygen, and, as he mentions, such a statement has an important bearing on the use of permanganate in volumetric analysis. I have repeated some of Jones's experiments, and while agreeing with him that in strong solution such an evolution of oxygen does occur, I am quite unable to find any evidence of the evolution of this gas with the dilute solutions I have mentioned. Moreover, the check experiments I shall shortly allude to confirm me in my opinion that no such fallacy can arise in this modification of the permanganate process. But if no oxygen be given off during the reduction of the permanganate, is none given off when one adds the sulphuric acid to the potassium salt (the permanganate)? I have estimated the amount of oxidising power possessed by a mixture of the permanganate and sulphuric acid solutions after standing for periods varying from a few seconds to 30 minutes, the reducing agent being a solution of ammonio-ferrous sulphate, and have found no diminution in strength. After boiling for some time a mixed solution of permanganate and sulphuric acid, and then estimating the oxidising power, there is certainly a marked but not very great diminution:—to 200 c.c. of distilled water, 50 c.c. of permanganate solution (1 per 1000) and 10 c.c. diluted sulphuric acid (1 in 10) were added, and the mixed solutions at once titrated against ferrous-ammonium sulphate solution, of which 53·25 c.c. were used. A similar mixture was gently boiled in a glass flask for three hours, when only 50·34 c.c. of the reducing agent were required to effect complete decoloration. Repetitions of the experiment gave very similar results. Further, in connection with the possibility of oxygen

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* *Journal of the Chemical Society*, vol. xxxiii. (new series), pp. 95–101.

being given off during oxidation of the substance used to reduce the permanganate, the solutions of permanganate and of ferrous-ammonium sulphate were separately poured into long glass tubes, which having been completely filled and closed with the thumb, were opened under water. The liquids gradually mixed by diffusion, but no evolution of gas could be detected. Oxygen, however, being somewhat soluble in water (roughly 3 volumes in 100 volumes of water), it may be questioned whether, in this last experiment especially, oxygen might not be given off, and be dissolved by the water. The permanganate and ammonio-ferrous solutions were separately shaken up with pure air, and after standing for some little time were mixed as before, and as before gave negative results.

The next point I wish to discuss is in what order should the various solutions be added? In all my experiments in reference to this question I have employed sufficient distilled water to dilute the the nitrous acid in the nitrite, to the extent of at least 1 in 5000, and in all the same bulk of water and the same solution of the nitrite—nitrite of sodium—were employed.

In this series, therefore—(1) I have added the nitrite first, then the acid, and immediately followed this by the permanganate; or (2) I have run in permanganate before the acid, and added this latter (*a*) slowly and (*b*) rapidly; and (3) finally, I have added the nitrite last, both (*a*) rapidly and (*b*) slowly. All these methods give fairly approximate results, but all (with one exception) allow some slight decomposition of the nitrous acid, evidenced by the odour of nitric peroxide, and by the amount of permanganate decolorised; this decomposition, judging from the odour and the results of the analysis, being especially marked where the sulphuric acid preceded the permanganate, the nitrite of sodium having been added first. The one exception was where the nitrite was slowly added, in a way I shall afterwards mention, to the previously acidified permanganate (3, *a*).

Again, as anhydrous nitrous acid decomposes ammonia into nitrogen and water according to the well-known equation,

$$2NH_3 + N_2O_3 = 2N_2 + 3H_2O,$$

does the presence of ammonia or its compounds give fallacious results with this permanganate process? Two beakers, each containing 50 c.c. of the permanganate and 10 c.c. of the sulphuric acid solutions, were taken—to one a considerable amount of sulphate of ammonium was added (as, of course, any ammonia or ammonium salt present would assume this form), and then to both an equal amount of nitrite of sodium solution. In each case precisely the same amount of ferrous ammonium sulphate was reduced. Moreover, solutions of nitrite of ammonium gave concordant results, and results which indicated that no secondary decomposition had occurred. Besides any nitrogen given off would almost certainly have been manifested by bubbles of gas.

These questions having been considered, we may now look to the details of the method. A tentative experiment, by the simple addition of sulphuric acid and permanganate solutions in Feldhaus' original way,* is done to roughly ascertain how much permanganate solution is required to oxidise the nitrous acid in 50 c.c. of the nitrite solution under examination, or the amount which would be required, were the nitrite pure, is calculated, which is the simpler plan, and one proceeds as follows:—Into a beaker of not less than 500 c.c. capacity, 200 c.c. or so of distilled water is poured. The required number of c.c. of permanganate for 50 c.c. nitrite solution being approximately known, twice or thereabouts that number is run into the beaker, and then 10 c.c. of the 10 per cent. sulphuric acid (or 12·c.c. of the pharmacopœial diluted acid) is mixed with this. Now *slowly* add 50 c.c. of the nitrite solution, the addition extending over about one minute, and the nozzle of the pipette being passed well below the surface of the liquid, and moved round so as to agitate the contents of the beaker. The nozzle is drawn above the surface when almost empty, and a little distilled water run over its exterior to wash off any adhering fluid from the beaker. After waiting two or three minutes (less time is really sufficient, as I find after one minute no trace of nitrite, but it is, as a matter of precaution, well to wait about three minutes) titrate back with the ammonio-ferrous solution to ascertain the amount of unchanged permanganate. The exact point of decolorisation is best obtained by zig-zag titration, a method of obtaining the end-point used with excellent results by Dittmar for his "Challenger" reports.† Now convert the quantity of ammonio-ferrous solution used into its equivalent in c.c. of permanganate, and in the ordinary way calculate the percentage of nitrite the sample contains.

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* *Vide supra.*

† "Challenger" Reports, "Physics and Chemistry," vol. i. pt. i. p. 4.

To test the method, nitrite of silver was precipitated from a moderately strong solution of nitrite of potassium by nitrate of silver, and two grammes of the recrystallised nitrite dissolved in lukewarm water were made up to one litre. As this nitrite is not very stable, the amount of silver was estimated by chloride of sodium, and calculating from this basis it was computed that every 50 c.c. of the solution contained ·02465 gram of anhydrous nitrous acid. The following six consecutive estimations show the quantities actually found :—

1. ·02464
2. ·02465
3. ·02463
4. ·02464
5. ·02465
6. ·02466.

The more commonly employed inorganic nitrites being that of those alkalies, I may here note that I find commercial specimens of nitrite of sodium, either in rods or crystals, usually contain nitrous acid equal to 94–95 per cent. of nitrite; specimens of nitrite of potassium an amount equal to from 86–88 per cent. of the salt; whilst nitrite of ammonium, being very deliquescent, is consequently sold in solution which generally contains, when not old, about 12 per cent. of actual nitrite.

In this paper, therefore, I have endeavoured to show—

1. That the permanganate process as ordinarily employed for the estimation of inorganic nitrites, viz., by gradually adding permanganate to the acidified solution of the nitrite, is unsatisfactory.
2. That it is desirable to use a large excess of permanganate, and titrate back with a reducing agent.
3. That very accurate results are obtained by using comparatively weak solutions of permanganate and of the reducing agent.
4. That the most suitable reducing agent is ammonio-ferrous sulphate, and that both it and the permanganate must not be in strong solution.

5. That the nitrite must be slowly added to the acidified permanganate.

6. That with the solutions recommended no fallacy is introduced by evolution of oxygen, or by secondary decomposition between ammonia or ammonium compounds and nitrous acid.

2. The Absolute Determination of the Strength of an Electric Current by means of the Balance. By Professor James Blyth, M.A.

The object of this paper is to describe a method of determining the strength of an electric current in absolute measure by measuring, in grammes weight, the repulsion between two circuits each carrying the same current.

For convenience of calculation, the circuits are made circles of equal radius, and are placed with their planes horizontal.

The construction of the instrument is as follows:—A delicate chemical balance is provided, with the scale pans replaced by two suspended coils of wire. Each of these coils is made of a single turn of moderately fine copper wire bent round a disc of glass of the requisite diameter. The ends of the wire, after being firmly tied together for a considerable length with a thin layer of insulating material between, are bent so as to lie radially along the upper surface of the disc. This disc is cemented concentrically to a similar disc of slightly larger radius, so that the wire is firmly fixed in the step formed by the two discs. The double disc has a round hole through its centre, by means of which it is attached to the end of a wooden cylinder. At opposite sides of this cylinder are fixed two vertical rods of brass of equal length, and terminating at the top in small platinum cups for holding mercury or dilute acid. To the lower ends of these rods, one to each, are soldered the ends of the copper wires which pass radially across the disc. The whole is suspended by a suitable hook from one end of the balance beam, and is so adjusted that the cups are in line with the knife edge at the end of the beam, and have their upper edges just a little above its level. A precisely similar coil is suspended from the other end of the beam, and the lengths and weights of both are so adjusted that, when the index of the balance is at zero, the coils hang with their planes exactly in the same horizontal plane. In order that the weighing may be at all exact it is necessary that the electrical connections, by means of which the current is led through the suspended coils, be so made as to interfere as little as possible with the sensibility of the balance. This is affected in the following manner:—An insulated copper wire, having its ends tipped with short lengths of platinum wire, is run along the lower edge of the beam, being firmly lashed to it by well-rosined silk thread. The ends of this wire, bent twice at right angles, are so placed that their platinum tips dip vertically into one of each pair of the platinum cups which are attached to the vertical rods of the suspended coils. From the other cup of each pair proceed two similarly tipped copper wires which run along the upper edge of the beam, and are also firmly tied to it. These wires only proceed as far as the middle of the beam, where they are bent first outwards, one on each side of the beam at right angles to it, and then downwards, so that the platinum tips are vertical. These latter dip into a pair of platinum cups attached to two vertical rods which spring from the base board of the balance. These rods are placed at equal distances on each side of the beam, and are of such length that the platinum cups are in line with the central knife edge of the beam, and with their edges just a little higher than its level. There are thus in all six cups and six dipping wires. Three of these are in line on one side of the beam and three on the other. Also the line joining the points of each pair of dipping wires is made to coincide with the corresponding knife edge; and further, the edges of all the cups are in the same plane when the balance is in equilibrium.

The fixed coils are made precisely in the same way as the suspended ones, with this exception, that the wooden cylinder with its vertical rods is removed. One of these is placed below one of the suspended coils (the right hand one usually), and the other, having a large central opening for the reception of the supporting cylinder and rods of the suspended coil, is placed above the other. Each is supported from the base board of the balance by three levelling screws, which also serve for adjusting the vertical distance between the fixed and suspended coils. This is done by means of three distance pieces of brass carefully worked so as to have the required length as determined by a screw gauge. To enable the respective pairs of coils to be placed concentric with each other, circles of equal radius are scratched on each glass disc, and the adjustment is made by looking from above downwards upon both discs from holes in the top of the balance case. The ends of the wires proceeding from the fixed coils dip into mercury pools formed in the base board of the balance, which also serve for making connections with the battery and the vertical rods which convey the current into the suspended coils. The connections are so arranged that the current circulates in opposite directions in each fixed and corresponding suspended coil. In this way repulsion is always produced, no matter how the current enters and leaves the entire circuit. The repulsion is measured by the number of grammes required to restore the balance to exact equilibrium.

From the above description it will be obvious that any motion of the beam in the act of weighing causes only a very slight motion of the platinum wires which dip into the fluid contained in the cups. In this way the resistance due to the viscosity of the fluid is very small, even in the case of mercury, and much smaller when dilute acid is used. In point of fact, the diminution of sensibility due to this cause is less than in the case of determining the specific gravities of solids by weighing in water in the ordinary way. With mercury it is quite easy to weigh accurately to a milligramme.

The repulsion between each pair of coils can be calculated from the formula given by Neuman for the action between two current elements.

Let a = radius of each coil.
x = the distance between their planes.
θ = the angle between the directions of an element ds of the one circuit, and in element ds' of the other.
r = the distance between these elements.

Then, if M denote the potential energy due to the mutual action of the two circuits when each carries unit current, we have

$$M = \int_0^{2\pi}\int_0^{2\pi} \frac{\cos\theta ds ds'}{r} .$$

Now $$ds = ad\theta\,,\ ds' = ad\theta'\,,\ r = \sqrt{x^2 + 4a^2\sin^2\tfrac{1}{2}\theta}$$

$$\therefore\ \mathrm{M} = \int_0^{2\pi}\int_0^{2\pi}\frac{a^2\cos\theta d\theta d\theta'}{\sqrt{x^2 + 4a^2\sin^2\frac{1}{2}\theta}}$$

$$\mathrm{M} = 2\pi a^2\int_0^{2\pi}\frac{\cos\theta d\theta}{\sqrt{4a^2 + x^2 - 4a^2\cos^2\frac{1}{2}\theta}}$$

$$= \pi ak\int_0^{2\pi}\frac{\cos\theta d\theta}{\sqrt{1 - k^2\cos^2\frac{1}{2}\theta}}\,.$$

If $$k^2 = \frac{4a^2}{4a^2 + x^2} = \sin^2\gamma\ (\text{say})$$

$$= 2\pi ak\int_0^{2\pi}\frac{(2\cos^2\frac{1}{2}\theta - 1)\,.\,d\,.\,\frac{\theta}{2}}{\sqrt{1 - k^2\cos^2\frac{1}{2}\theta}}$$

$$= 4\pi ak\int_0^{\frac{\pi}{2}}\frac{(1 - 2\sin^2\frac{1}{2}\varphi)d\,.\,\frac{\varphi}{2}}{\sqrt{1 - k^2\sin^2\frac{\varphi}{2}}}$$

If $$\theta + \varphi = \pi\,.$$

$$= 4\pi ak\int_0^{\frac{\pi}{2}}\frac{\left\{1 + \frac{2}{k^2}\left(1 - k^2\sin^2\frac{\varphi}{2} - 1\right)\right\}d\frac{\phi}{2}}{\sqrt{1 - k^2\sin^2\frac{\varphi}{2}}}$$

$$= 4\pi ak\left[\int_0^{\frac{\pi}{2}}\frac{d\,.\,\frac{\varphi}{2}}{\sqrt{1 - k^2\sin^2\frac{\varphi}{2}}} - \frac{2}{k^2}\int_0^{\frac{\pi}{2}}\frac{d\,.\,\frac{\varphi}{2}}{\sqrt{1 - k^2\sin^2\frac{\varphi}{2}}} + \frac{2}{k}\int_0^{\frac{\pi}{2}}\sqrt{1 - k^2\sin^2\frac{\phi}{2}}\,.\,\frac{da}{2}\right]$$

$$= 4\pi ak\left\{\mathrm{F}k - \frac{2}{k^2}\mathrm{F} + \frac{2}{k^2}\mathrm{E}\right\}$$

$$= 4\pi a\left\{\left(k - \frac{2}{k}\right)\mathrm{F} + \frac{2}{k}\mathrm{E}\right\}.$$

When F and E are the first and second complete elliptic integrals to modulus *k*.

To obtain the repulsion we must differentiate M with respect to x. This gives

$$\text{since} \quad \frac{dk}{dx} = -\frac{k^3x}{4a^2}$$

$$\text{and} \quad \frac{dF}{dk} = \frac{1}{k(1-k^2)}\left\{E-(1-k^2)F\right\}$$

$$\text{and} \quad \frac{dE}{dk} = \frac{1}{k}(E-F)$$

$$\frac{dM}{dx} = \frac{\pi}{a} \cdot \frac{kx}{1-k^2}\left\{(2-k^2)E-(2-2k^2)F\right\} \cdot$$

By substituting $\sin^2\gamma$ for k^2 this becomes

$$\frac{dM}{dx} = 2\pi\cos\gamma\left\{(1+\sec^2\gamma)E-2F\right\} \cdot$$

One of the instruments exhibited has two pairs of coils. In it

$a = 10$ centimetres.
$x = 1{\cdot}048$ „

$$\therefore\ k = \sin\gamma = \sin 87^\circ.$$

$$\begin{aligned} E &= 1{\cdot}005258587 \\ F &= 4{\cdot}338653976 \\ \cos\gamma &= {\cdot}0523360 \\ \sec\gamma &= 19{\cdot}107323 \end{aligned}$$

$$\text{let } 2\frac{dM}{dx} \cdot \frac{1}{g} = G$$

$$\therefore \log G = \bar{1}.3818437 \ \therefore\ G = {\cdot}24090.$$

The other instrument has only one pair of coils. In it

$a = 15{\cdot}3008812$
$x = 1.$

The value of G is calculated in the same way.

3. On the Pelvic Girdle of Birds and Reptiles. By Professor D'Arcy Thomson.

4. On Sulphines. By Orme Masson, D.Sc.

5. On the Nature of the Relationship of Urea-Formation of Bile Secretion. By D. Noël-Paton, M.B., C.M.*

6. Note on the Collisions of Elastic Spheres. By Professor Tait.

(*Abstract.*)

In my paper of January 18th (*anté*, p 537) I had assumed all directions of the line of centres at impact to be equally probable. The introduction of the true condition affects the average value of

$$u^2 - uv$$

by a numerical factor only; so that the proof of Maxwell's Theorem remains valid. The expressions for the separate average values of u^2 and of uv, however, are altered more profoundly.

PRIVATE BUSINESS.

Sir William Thomson gave notice of the following Motion, to be considered as part of the Private Business on the evening of 19th April:—"That henceforth the Meetings of the Royal Society be held in the Afternoon instead of at 8 P.M."

Mr John Murray gave notice of an Amendment to the Motion of Sir William Thomson, in the terms, "That the First Meeting in each Month take place at 4 P.M."

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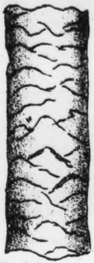
* This paper appears in full in the *Journal of Anatomy and Physiology*, vol. xx. p. 520, 1886.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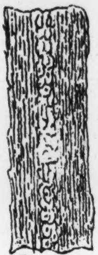
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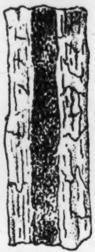
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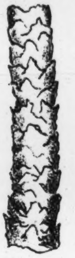
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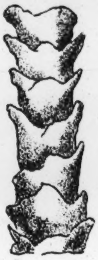
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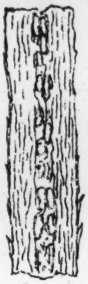
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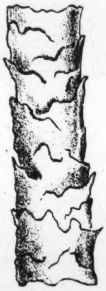
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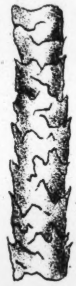
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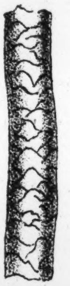
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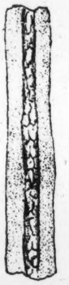
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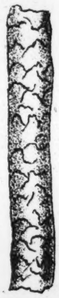
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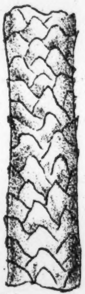
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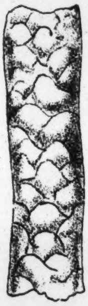
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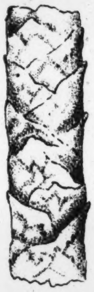
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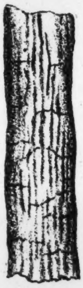
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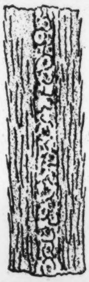
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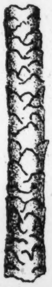
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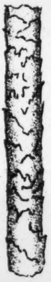
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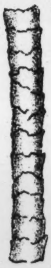
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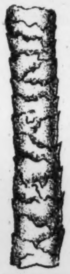
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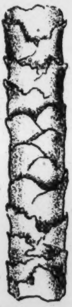
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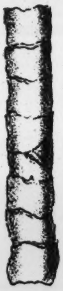
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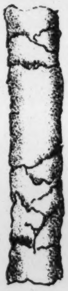
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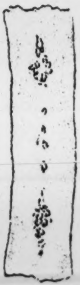
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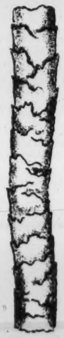
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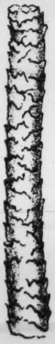
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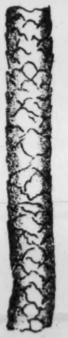
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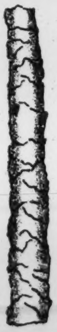
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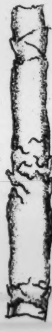
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McFarlane & Erskine, Lith^{rs} Edin^r